VAN NOSTRAND'S

ECLECTIC

ENGINEERING MAGAZINE.

NO. CXIX.—NOVEMBER, 1878.—VOL. XIX.

ON THE PROPOSED REMOVAL OF SMITH'S ISLAND.

By Prof. LEWIS M. HAUPT.

Read before the Engineers' Club of Philadelphia.

The commercial interests of Philadelphia have developed to such an extent as to create a demand for greater wharfage facilities with deeper water; and that cereals and merchandise may be delivered without too many handlings it is advisable that cars should be run immediately alongside the vessels to be laden. To accomplish this it is proposed to lay tracks on Delaware Avenue, already too narrow, and to make provision for the space thus occupied by extending the Port Warden's line farther out and thus contract the river channel now only about 800 feet wide at the narrowest part. Several of our largest shippers have requested permission to extend their wharves several hundred feet. Were this to be allowed in a few isolated cases it would introduce dangerous barriers to navigation, and if an advance be made all along the line it would seriously contract the channel, unless a portion of Smith's Island can be removed.

The project is by no means a physical impossibility, as much larger deposits have been successfully taken away. The work of improving the river Neva in Russia is one of far greater magnitude as the following clipping from the *Ledger* witnesseth:

"Following the large order from Russia for Philadelphia locomotives comes the information that the Russian Government has just concluded, through Major W. R. Bergholz, a contract with the Morris & Cummings Dredging Company of New York, for deepening to a uniform depth of twenty feet the channel of the river Neva, between Cronstadt and St. Petersburg. Twenty-five thousand dollars were cabled to Russia last week as earnest money. The dredging 'plant' will cost $200,000. Most of it will be constructed in this country, and will be on hand ready for operation on first of May next. The quantity of mud, etc., to be excavated is estimated at 15,000,000 cubic yards, and the work must be completed in four years. (The contract was obtained after sharp competition with English operators.)"

To widen the Ship Channel of the Delaware River 1000 feet along the Smith's Island front, and to a depth of 18 feet, would require the removal of only about 5,000,000 cubic yards of material at a cost of about $1,000,000.

The same width and depth of channel may be obtained if desired, for less than $\frac{1}{10}$ the cost of dredging, by a careful adjustment of the *regimen* of the river by auxiliary constructions such as jetties, rip-raps, sand fences or bottom-dams. Before these structures can be located precisely, it will be necessary to make a

careful examination or survey of the river to determine its surface and mean velocity, the nature of its bed, its cross section, the directions of its banks and currents, whether straight or sinuous and its longitudinal slope. These quantities are evidently functions of each other, and together constitute what is known as the *regimen* of the river. So mutually dependent are they that a change in any one will affect them all.

The tendency of rivers is to maintain a constant *regimen*, and this fact is the key to the solution of many problems relating to river improvements.

All fresh water flowing through alluvial deposits carries with it in suspension more or less earthy matter. We find, therefore, a continual tendency to deposit where the velocity is least, and to scour where it is greatest, and this mechanical action of water is constantly pushing the river bed downwards to the sea. It is estimated that the "Mississippi annually transports to the Gulf a volume of alluvion one mile square and 241 feet high, weighing over 400,000,000 tons, and at the same time it pushes over the bar at its mouth an amount equal to $\frac{1}{10}$ of that sum," making altogether over 272,000,000 cubic yards. This is far beyond the limits of our present mechanical possibilities. Thus the river furnishes its own motive power, gathering up its load as it rolls along, and dumping it at the end of its course, not always, it is true, just where it is desired, unless the spot be indicated by depositing some obstruction, in which case it will not fail to notice the sign "dirt wanted here," and continue adding until its *regimen* is re-established, when it will move on as before.

Let us assume a straight length of river-bed of uniform cross section, a certain fixed stage of water and inclination, direction and nature of bed, and we will find the discharge will be constant, or the water and its suspended earthy particles will move on with a uniform velocity, some being deposited, it is true, while others are pushed along or gathered up; but the mean velocity of the parabola representing the wave front will remain uniform. So soon, however, as the above relations are disturbed, the effect becomes at once manifest. Suppose, for example, the cross section be increased; the velocity would be reduced, and, consequently, the carrying and scouring capacity being limited, deposits would be formed; or if a bend be introduced, it would retard the threads of the current on its side of the stream, whilst those of the opposite side, flowing faster, must return to fill the vacuum which would otherwise be created, and thus be drawn over towards the bend to receive a new impulse from the inner threads, and by these constantly recurring differences of velocities cause the alluvium to be precipitated.

Again, should one stream intercept another of lesser volume, the mouth of the latter would become choked up with a bar, in consequence of the reduced velocity of its currents, which will then spread out laterally in the effort to maintain a constant discharge, and so form deltas. For this reason, I do not believe the improvement at the South West Pass to be a permanent one. The effect will ultimately be to elongate the bar into the deeper water of the Gulf, but the extension will be so gradual that the expense of maintaining an open channel will be very slight.

On the other hand, anything tending to reduce the cross section and so increase the velocity or discharge will produce a scour, and unless the bed be of rock or hard pan, will deepen or widen the channel. Such contraction may be accomplished in two ways, either laterally by drawing in one or both banks, or vertically by filling up the bottom to a limited height.

As a consequence of the principles just enunciated we will find in an alluvial bed that where the distance between the banks is least the channel is deepest; where greatest it is shallowest, or bars are most numerous; where points jut out, forming elbows, there will invariably be a shoal on the lower convex shore, whilst on the opposite or concave side will be found the best channel; that at the efflux of a lake, or broad expanse of river, where the several currents assemble before a final shoot through the contracted water-way, there will be deposits, and that at the mouths of rivers emptying into running water or beaches exposed to the winds and waves, bars will be formed, sometimes to such an extent as entirely to interrupt navigation.

Indeed, on the south shore of Lake Superior I have walked over the mouths

of some small streams without suspecting their presence, and only discovered them by exploring inward.

With a knowledge of these principles it is possible to predict with almost absolute certainty just where shoals may be found by a mere inspection of the outlines of the stream.

The tendency of an elbow to cause deposits is one which constantly increases, so that the bar creeps up stream to meet the elbow and ultimately joins itself to it, forming a spit. This so greatly reduces the water-way as to cause erosions at other points that the regimen may be preserved and thus new channels are cut through. Hence the fickleness of rivers with low, earthy banks.

But to return to the application:

Smith's, or more more properly Windmill, Island is represented, so far back as we have any authentic data, considerably farther down the river than at present, and it has been gradually creeping up stream, until now its upper end is about opposite Chestnut Street. To corroborate the above theory I have examined the oldest obtainable maps in the Mercantile Library, Pennsylvania Historical Society, Philadelphia Library, City Engineer's Office, and Franklin Institute, with the following results:

The map of Thos. Holme, Surveyor General of the Province, 1681, shows a small island opposite Spruce Street, and another much larger about opposite Kaighn's Point.

In 1762 Windmill Island extended from below Christian to below Spruce Street, with bars all the way up to Cooper's Point. (No name to map.)

The map of Scull & Heap, 1777, gives about the same position for the island.

On the map of 1796 the island extends from below Shippen (now Bainbridge) Street to below Chestnut, with a shallow channel across it opposite Spruce Street; or, in other words, a shoal showing above water between Spruce and Chestnut Streets, but not yet joined to the body of the island.

Hill's map, 1808, represents six small islands or flats dry at low water extending from Christian to Vine.

In 1811, the island extended from between Shippen to between Market or High Street, with bars at each end, the upper one being attached to the island, the lower reaching to Washington Ave.

The map of a survey by Jno. A. Paxton, and drawn by Wm. Strickland, Engineer (1824), shows three islands extending from Catherine to Arch Streets with shoals at either end.

Port Warden's map (1836) having no date other than that of its presentation to the Franklin Institute, and no name, shows the upper end of island reaching above Chestnut Street with isolated upper bar extending to Arch Street. The lower limit is not defined. (No canal shown.)

On the map of F. I. Roberts (1838) the island extends from Shippen to above Chestnut Street with a separate shoal reaching as far as Arch Street, and a shoal below from Washington Avenue to above Christian. (Canal shown as cut through.)

Map of Chas. Ellet, Jr. (1839); island from South to between Market Street (with canal) and isolated bars above and below, the latter reaching from below Washington Avenue to Fitzwater Street, the former to Cherry Street. Total length with bars, $1\frac{3}{5}$ miles.

The U. S. Coast Survey map (1843) shows the island as extending from Shippen to between Market, with ferry canal cut through, also a detached bar below, dry at low tide; one fathom depth just above Washington Avenue, and an *attached* bar on the up-stream end extending to Cherry Street, with one fathom of water below Callowhill Street.

The Surveys of Richard Hexamer (1868) limit the island by the prolongation of South and Chestnut Streets; and Dyer's map of 1869 makes it reach from Shippen nearly to Arch Street.

Of all these the only maps giving any information concerning the depths are those of the U. S. C. S., made in 1843—and the Port Warden's map having no date affixed—and, consequently, the only one upon which any reliance can be placed is that of 1843. Still a general comparison of all shows an average movement of the lower end of the island up stream from Christian to South Street, a distance of 1900 feet in 106 years, or from 1762 to 1868.

From the comparative soundings of 1819 and 1836 as given on the Port Warden's Map and those of the Coast

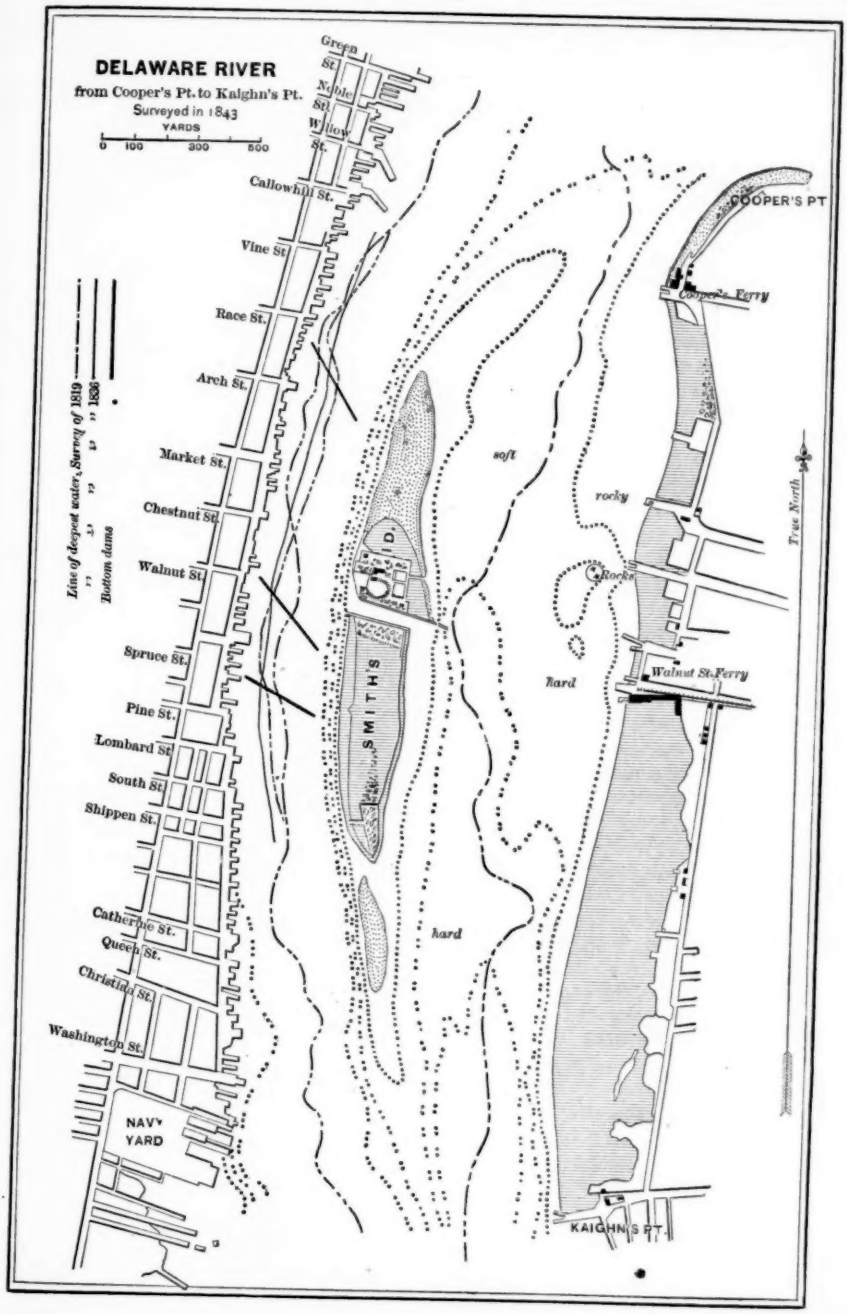
Survey of 1843, we are enabled to trace in *plan* the axes of the deepest water at those dates with the following notable results. In 1819 the axis was 250 to 300 feet from the Port Wardens line and very nearly parallel thereto. In 1836, after 17 years, it had evidently moved slightly towards the City shore, and in 1843 was still nearer from Race Street to Chestnut Street, approaching to within 90 feet of the pier heads at Market Street. At Chestnut Street it made a bend, convex towards Smith's Island, having its maximum ordinate opposite Walnut Street, and remained outside the lines previously occupied to beyond the limits of the maps.

Theory would suggest that as the approach to the island happened just opposite the canal cut for the Philadelphia and Camden Ferry Company, it must have resulted from the set of the current in that direction, and as there is a corresponding flexure of the deepest water line in the Jersey channel it corroborates the theory.

A search for the date of the opening of the canal resulted in a note from Mr. Thompson Wescott to the effect that "the work was authorized by Act of Council, Feb. 14, 1838, and damages assessed the same year @ $2000. The Canal, 150 feet wide, was cut soon afterwards," he supposes in 1838–9. At first, both sides of the canal were of the same length, in consequence of which it filled up rapidly, but by extending the upper side into the Jersey channel to intercept the flood tide and the lower side into the Pennsylvania channel, to catch the ebb, and cause a scour, it has since been kept open. The survey of 1843, four years after the opening of the canal, shows a very marked effect upon the axes of the currents. An examination of the *profile* shows 29 feet opposite the old Navy Yard, near the lower end of Shoal, below the island. Thence the depth increases with undulations to 58 feet at a point above Race Street, at the upper end of the shoal above the island (distance 6800 feet), whence it suddenly shoals to 31½ feet opposite Cooper's Point (distance 3200 feet), at which place the river is widest.

It deepens again to 37 ft. opposite lower end of Petty's Island, and shoals gradually to a point above the Reading Company's wharves where there are but 19 feet of water, thence the depth increases to 26 feet at head of island, and, finally, runs up to only 13 feet, just below Fisher's point, where it pitches down suddenly to 38 feet.

Returning by the Jersey channel we find the distance somewhat greater, by the deep water line, because it is more sinuous in consequence of the greater width of channel and less depth of water. The same general observations obtain in this case as in the other, *i. e.*, where the river is broadest it is shallowest and *vice versa*. Considering the profiles of the two channels together, we find, as a rule, the average depth greatest where the breadth is least, and the reverse, so that we may safely conclude from these (observations and deductions) that if by any means the breadth or depth be reduced the depth or breadth will be increased in consequence of the scour produced by the increased velocity given to the stream. This diminution of the sectional area may be produced either laterally by constructing jetties and levees, or vertically by forming subaqueous dykes or dams on the bed of the stream, and crossing the same either directly or obliquely. The latter being generally better as it will change the direction of the resultant thread of the current so as to cause it to act more powerfully on the deposits to be removed. In applying these principles to the case in point, I should recommend the latter method of reducing the water-way by oblique dams (see map) constructed, first of large stone thrown into the river on range lines established by signals erected on the island, and filling in on the up-stream side with rip-rap or ballast from vessels. The Penna. end of the dam should be somewhat higher than that resting on the island, and no part of it should have less than thirty feet of water over it at mean low tide. As an auxiliary structure I should extend the pier heads near Willow Street (see map) down stream, at such an angle as to deflect the current towards the head of the island, and believe, that by thus expending a few thousand dollars, the present channel may be so deepened and widened, as to avoid entirely the *removal* of the island. At present I do not think it advisable to remove any of the fast land


DELAWARE RIVER
from Cooper's Pt. to Kaighn's Pt.
Surveyed in 1843
YARDS
0 100 300 500
Green St.
Noble St.
Willow St.
Callowhill St.
Vine St.
Race St.
Arch St.
Market St.
Chestnut St.
Walnut St.
Spruce St.
Pine St.
Lombard St.
South St.
Shippen St.
Catherine St.
Queen St.
Christian St.
Washington St.
NAVY YARD
Line of deepest water, Survey of 1819 — 1838
Bottom dams
SMITHS
soft
hard
rocky
Rocks
hard
COOPER'S PT
Cooper's Ferry
Walnut St. Ferry
True North
KAIGHN'S PT.

which is now sufficiently protected by a casing of piles; but, on the contrary, I believe it would work serious injury to the harbor were any very considerable part of the island to be removed, as in that case the deep water channel would recede from the Penna. shore where bars would soon form and destroy the approach to the harbor. It is also serviceable as a breakwater, besides furnishing so much more room for stowage and wharfage which are as essential to commercial interests as good water.

I do not believe the time has yet arrived when it will pay to pull up the piles now surrounding the island, and set them further back, but I do think it would be expedient to deepen the channel close up to the present wharf lines on the island by the inexpensive method proposed.

The question will naturally arise as to the effect upon the lower reaches of the river from the alluvium thus disturbed. It is my opinion that it will not seriously affect the present navigable channel, but it will doubtless add to the magnitude of the bars already existing below Greenwich, Gloucester and Red Bank.

As to the time required to effect these changes it is impossible to make any predictions with certainty, for it will depend largely upon the stages of water, and be retarded to a considerable extent by the flood and stand of the tide, but it will doubtless improve the channel, at least as rapidly as the demand for greater shipping facilities increases.

A new survey of the river is now being made by the U. S. C. S., under the supervision of Capt. S. C. McCorkle, the results of which will be looked for with great interest, as indicating more correctly than can be done by other means the exact location of any proposed improvement.

WATER SUPPLY TO A STAMP MILL IN VENEZUELA, WITH NOTES ON KUTTER'S FORMULA.

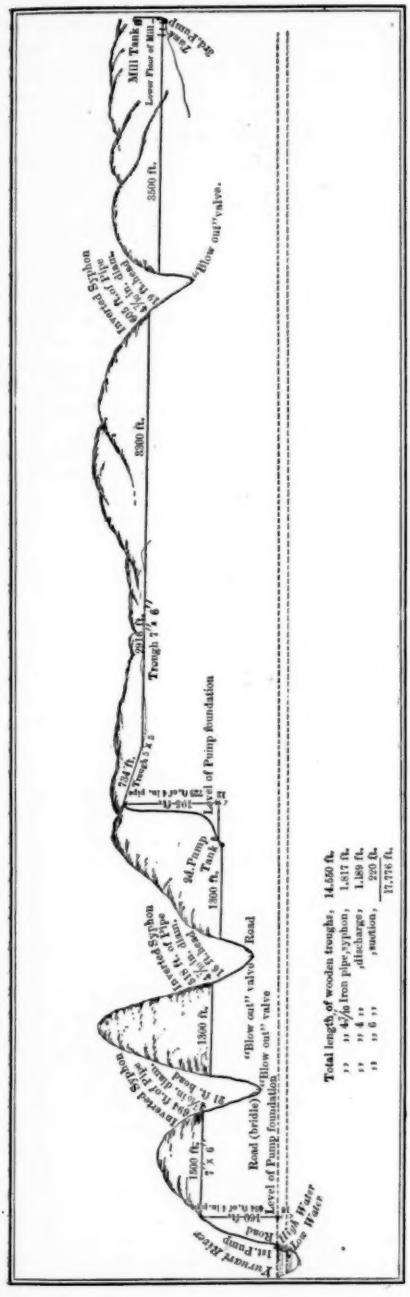
By WM. A. BIDDLE.

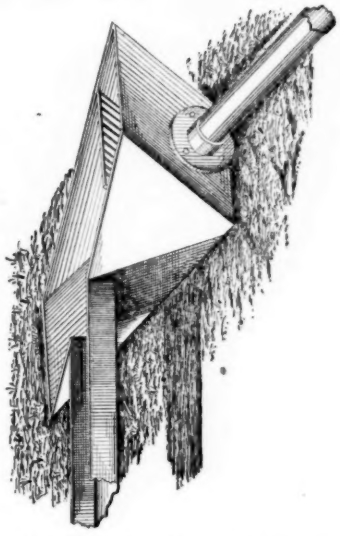
From a Paper read before the Engineers' Club of Philadelphia.

In making the necessary calculations for the location and construction of works to supply water to a quartz mill in the gold region of Venezuela, South America, the wide differences between the formulas given by well-known authorities for the flow of water in pipes and open channels became very apparent, particularly when applied to comparatively small dimensions. This mill of thirty stamps and the general plant of the company owning it, had previously been built close by the outcrop of the quartz vein and almost three miles from the nearest stream, in the disappointed expectation, on the part of the gentlemen then managing, of getting a supply of water by sinking to a moderate depth on the vein.

In order to show the conditions to which the formulas were applied, and also as illustrating some of the peculiarities met with in that country, a few descriptive notes are given of the works referred to.

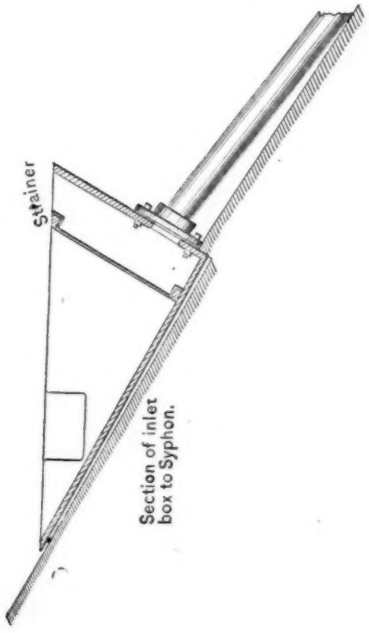
These consisted (see Profile) of a pumping station at the foot of a steep hill on the Yuruari River (an affluent of the Essequibo), delivering water 160 feet above the pump into a line of troughs (7×6 inches inside, made of inch boards) laid along the hill sides on a descending grade of .3 per 100 for a length of 4,100 feet, the line crossing two deep ravines by inverted syphons (of boiler flues five inches diameter outside) 694 feet and 518 feet long, bringing the water to the second pumping station at the foot of a range of hills extending inland, whence the water was delivered 195 feet above the pump into a second line of troughs 10,450 feet in length—this line crossing another ravine by an inverted syphon 605 feet long—bringing the water into a ravine immediately below the stamp mill, whence a third pump run from the mill boilers delivered it into the mill tank; the total surface length of the line, including the section and discharge pipes of the pumps, being 17,300 feet, and the





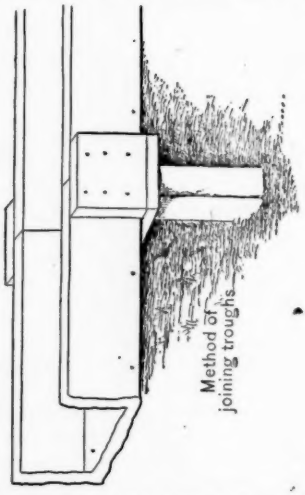
total height gained from the river to the mill tank being 310 feet.

The pumps at the two stations were Worthington's Duplex, 16-inch steam cylinders, 8-inch plungers, and 10-inch



stroke, with 6-inch suction and 4-inch discharge pipes. The boilers were of locomotive pattern, having forty-five 3-inch flues eight feet long, and the exhaust of each pump was led into the smoke stack of its boiler. Check valves were placed in the discharge pipes close to the pumps, and inch pipes were tapped in just above the valves and leading to the boilers, which were thus fed by the pressure of the water column, though having injectors for use in case of necessity.

The boards for the troughs were sawed at the company's sawmill, close by the stamp mill. The durable native woods, with one or two exceptions which are of very rare occurrence, are extremely hard and heavy. The boards come from the saw quite smooth, but it is almost impossible to drive a nail near the edge without splitting the wood, and, therefore, the side boards of the troughs were bored for the nails by a machine fitted up for the purpose in the saw mill.



The troughs varied in length from twelve to sixteen feet, and were so stiff and strong that no supports were needed between the joints.

The pumps, boilers and fixtures, pipes, pipe fittings and tools, valves, bends, bolts and nuts, nails, indeed everything used in and on the work except the boards, had to be shipped by sailing vessels from New York up the Orinoco River some 300 miles, landed by lighters, loaded on ox-carts, and hauled 150 miles inland to the mines. Fortunately both pumping stations were close to the cart roads, but many of the syphon pipes had to reach their destination among the hills by being packed on donkeys.

The preliminary grade line for the troughs was run with a builder's level, or triangle, eight feet long and made of boards. This was really the quickest and handiest instrument that could be used, for almost every foot of the distance had to be cut through the dense tangle of vines, briers and lianas which form the undergrowth of the tropical forests, and the amount of chopping was thus reduced to an opening just sufficient to drag the triangle along, while by driving pegs and keeping "tally" both the measurement and the grade line were obtained in the one operation with enough precision for preliminary work. The final leveling, after the line had been approximately located and cleared, was done with a "Heller & Brightly" small mining level, which proved a most satisfactory instrument.

In calculating the heads to be given to the inverted syphons for a maximum discharge of thirty-five cubic feet per minute, two formulas were applied, Weisbach's for friction head (velocity head to be added), and Eytelwein's as given by Trautwine for total head, and also by Beardmore; with the following results:

	Feet long.	Eytelwein.	Weisbach.	Diff.
1st. Syphon,	694	19.09	14.67	4.42
3d. "	605	16.71	12.83	3.88
2d. "	518	14.39	11.04	3.35
				11.65

Those by Eytelwein being thirty per cent. greater than those by Weisbach. In the absence of any record of the use of such small pipes (4.7 inches inside) as inverted syphons, it was thought wiser to take the larger results though involving a greater loss of elevation by almost twelve feet, and also to add two feet for bends and possible obstructions in the pipes, so that the heads actually given for the above lengths were twenty-one feet, nineteen feet, and sixteen and a half feet respectively. Trautwine remarks on this subject as follows:

"Recent experimenters state that the old formulae in use, though generally

sufficiently exact for ordinary practice, are to some extent defective. Weisbach asserts that for velocities less than 1½ feet per second (full one mile per hour) the heads given by the other formulae are too small; and for higher velocities too great. On the other hand many measurements by competent engineers seem to show that the old formulae give all the accuracy required in common practice."

The first trial of the works, and unfortunately the only one made before the engineer left the country, included only the first pumping station, 1,500 feet of troughs and the first syphon, and was made under circumstances which rendered it impossible to test the performance of the syphon further than ascertaining that the 22 cubic feet per minute, then estimated to be flowing through the troughs, passed the syphon with no indication of filling the high side. The three syphons have now been in use nearly two years, but the only information yet received about them states, that when the works are furnishing more water than the mill needs the syphons show no sign of filling the high sides. This proves that the formula used was certainly *safe* in this case, but it is hoped that further details will soon be received by which to learn how much it is in *excess* of safety, and whether Weisbach's formula *might* have been safely used, since an unnecessary loss of twelve feet of elevation could hardly be considered by Mr. Trautwine as "sufficiently exact for common practice," and sometimes might be of very serious importance.

At the trial, during which the pump was run slowly, the water flowed in the troughs three inches deep, and a small piece of inch board floated through the 1500 feet in 9¼ minutes, or at the rate of 2.7 feet per second. If this was the true surface velocity, then taking the ratio between the surface and mean velocities at .85, the mean velocity would have been 2.3 feet per second, giving a discharge of twenty cubic feet per minute. But the float was of such heavy wood that it was immersed its entire thickness, thus having its under side only two inches from the bottom of the trough, and there can be no doubt that if a strictly *surface float*, such as a thin disc of light wood, had been used, a considerably greater velocity would have been shown. Moreover the line of troughs in following the grade along the contour of the hillsides had almost constant changes of direction at the joints, while the formulas for discharge through open channels are given for *straight* channels, so that in order to compare them closely with the observed result in this case a correction should be applied to the result both for thickness of the float and for crookedness of the channel.

The differences between the formulas, both older and more recent, that were tried on this case, are in the values given to the co-efficient C in the formula for mean velocity, in feet per second,

$$V = C\sqrt{RS}$$

in which R is the hydraulic mean radius (area of water section divided by its wet perimeter), and S is the fall in one unit of length. Here the water section was 7×3 inches, or .58×.25 feet=.145; and $R = \frac{.145}{.25+.58+.25} = .134$. The fall being .3 per 100,

$S = .003$, and $\sqrt{RS} = \sqrt{.134 \times .003} = .02$

Beardmore gives for ordinary use,

$$V = 94.2\sqrt{RS}$$

And for "channels constructed with great care and straight in direction,"

$$V = 100\sqrt{RS}$$

The former gives in this case a mean velocity of 1.88 feet per second, and the latter two feet, corresponding at 85 per cent. to surface velocities of 2 2 and 2.35 feet per second respectively—both much below the observed result even without correction.

Weisbach gives 92.5 as the co-efficient of $\sqrt{RS}$, and other authorities vary from 68 to 100.

Bazin gives four different co-efficients for different degrees of smoothness in the material of the channel, all including the hydraulic mean radius as a factor, and the greatest being, for smooth plank (Higham's tables),

$$V = \frac{1}{\sqrt{.0000457\left(\frac{R+.098}{R}\right)}}\sqrt{RS}$$

This, applied to the case in question,

gives a co-efficient of 112.36, and a mean velocity of 2.25 feet per second, corresponding at 85 per cent. to a surface velocity of 2.64 feet per second—still below the observed result even without correction.

Kutter's co-efficient includes as factors both the hydraulic mean radius and the inclination, and also a "natural constant" depending on the material, and for which a table of values is given, varying from .009 for smooth plank to .035 for rivers and canals full of weeds and stones. The formula is thus (Higham's tables)

$$V=\frac{\left(41.6+\frac{1.811}{N}+\frac{.00281}{S}\right)\sqrt{R}}{\sqrt{R}+N\left(41.6+\frac{.00281}{S}\right)}\sqrt{RS}$$

Taking the value of N for smooth plank = .009, this gives for the case in question a co-efficient of 119.145, and a mean velocity of 2.383 feet per second, corresponding at 85 per cent. to a surface velocity of 2.8 feet per second, which may be considered as agreeing closely with the observed result of 2.7 feet per second corrected for thickness of the float. But as this result was obtained in a channel very far from straight it would seem that even Kutter's co-efficient is slightly below the truth for this case. It is, however, very close, and much nearer than that of Bazin, which has been thought accurate when applied to small channels, though acknowledged to fail on large rivers.

According to Kutter's formula a depth of .4 feet (say 4¾ inches) of water in the troughs would have a mean velocity of 2.82 feet per second, which would give the maximum discharge of 35 cubic feet per minute, assumed in calculations for the line, with a surplus velocity of 3.32 feet per second.

The English translation of Kutter's work (by L. D. A. Jackson, A.I.C.E.) gives an interesting account of his investigations, in which a great number of recorded observations, as well as his own, were tabulated and compared in various ways and with most laborious research. Without going fully into the mathematical details, it describes the *method* of deriving the new co-efficient, which may be said to consist largely of a synthetic application of analytical geometry, by plotting the observed co-efficients as ordinates, to abscissas representing values of R, and to others representing values of S.

It is claimed that this new formula gives co-efficients of $\sqrt{RS}$ which will be found correct whether applied to a petty drain or an immense river. The formula of Humphreys and Abbot for *large* rivers had been accepted as the best yet proposed, but their modification of it for *small* streams, when applied to small channels with considerable inclinations, is said to fail as completely as that of Bazin on large rivers. But Kutter's formula is said to have been proved on the great depths and low inclinations of the Mississippi, and to have given co-efficients equal to those found there by Humphreys & Abbot's observations, which have gone as high 254.4. This and its close agreement with observed results in the case of the small trough which has been described, certainly seem to justify the claim made for it and entitle it to the confidence of engineers.

Kutter's investigations have demonstrated the following important and interesting facts: that for a constant value of N, when the hydraulic mean depth (R) is one metre, the co-efficient is practically the same at all inclinations; that with values of R *greater* than one meter, the *co-efficient increases* as the *inclination decreases*, an extreme case of this being the very high co-efficients for the Mississippi; while with R *less* than one meter, the co-efficient increases as the inclination increases up to S=.001, beyond which point any further increase of inclination has practically no effect on the coefficient, which then varies only with R.

In the preface to the English edition of Kutter, the translator alludes to the anomalous fact that "the English-speaking races," while taking the lead in engineering progress in other directions, have been very far behind in hydraulics, one evidence and consequence being that this book which appeared in Austria, Germany, and Switzerland, in 1870, and was *immediately* translated into French, Dutch and Italian, was not published in England until *six years later*, and that too in spite of costly experience in the irrigation works in India of the necessity

of more knowledge in this branch of science. An extract is also given from an article in *Engineering*, Dec. 31, 1875, which says that Neville's tables of velocities based upon Dubuat, "though expressed in hundredths of an inch, are in reality but the wildest guesses at the actual velocities in irrigation canals of ordinary dimensions. Col. Cautley relied upon Dubuat when he laid out the Ganges Canal, and found him but a rotten reed, for the water in every instance tore along at an unexpected velocity, and erosion of the bed and destruction of the works followed." The writer of this article then sets aside as unreliable for such work almost all the familiar text books, both original and compiled, Continental and English, down to the time of D'Arcy and Bazin. If engineers in England have been behind the age on this subject, it is to be feared that we in America have been more so, for the Continental scientific journals of Europe (in which Kutter's work was first published) are less known and read here than in England, and are hardly enough "quoted" in our own periodicals to keep the profession at large well posted on the progress in those countries—else some of our lately issued "Hand Books" would have contained Kutter's very important results.

Kutter's *Tables* are in metrical measures, and are therefore not so convenient for use here at present, as it is to be hoped, they may be some years hence. A smaller but more comprehensive set of tables for open channels has been calculated in English feet from both Bazin's and Kutter's formulas, by Thomas Higham, Engineer of Irrigation Works in the Punjab, India, which can be recommended as convenient for use and reliable.

FRICTION BETWEEN A CORD AND PULLEY.

By I. O. BAKER.

Written for Van Nostrand's Magazine.

The method of operation, in the experiments herein detailed, was to suspend known weights to each end of a cord passing over a fixed drum, and measuring the friction directly by adding weights enough to overcome the friction. The apparatus was so arranged that the arc of contact between the cord and drum could be varied from 0° to 360°. This was accomplished by arranging an arm, which carried a pulley, so as to revolve about the drum. Separate observations were made to eliminate the friction of the pulley. In the course of the work some difficulty was found in determining exactly when the friction and added weight were in equilibrium. In all cases the mean position was the one sought. The co-efficient was computed by the well-known formula, given on page 617 of Rankine's "Analytical Mechanics," which, stated in words, is: "the ratio of the tensions of the free ends of the cord equals the base of the Naperian logarithms raised to a power indicated by the product of the co-efficient of friction and the arc of contact measured in terms of the radius."

The first series of experiments was was made upon an oak drum 4.09 inches in diameter, which had been turned in a lathe and finished with medium fine sand-paper. The cord used was a hard twisted, three strand, cotton cord, 0.08 of an inch in diameter. The arc of contact varied from 0° to 360° by steps of 10° each. All necessary corrections were made and the co-efficient computed for each angle. The results vary between .2319 and .1312, the mean of the thirty-six observations being .1599. Up to 30° the co-efficient diminished quite rapidly, while from 30° to 360° it decreased slowly as the angle increased. This is accounted for by the fact that the cord became harder under the increased tension. If we neglect four results, which vary more widely from the mean (owing probably to errors of observations) the limits then become .1679 and .1412, and

the mean .1563. The observations discarded are all from small angles.

In the second series, the conditions were the same as in the first, with the exception of the substitution of a drum whose diameter equals 1.81 inches. It was noticed that in this experiment the data agreed approximately with that of the first series to about 140°, hence the co-efficient was computed only for the twenty-one angles between 140° and 360°. The range in this case being between .1413 and .1265 and the mean .1371. For the same angles in the first series we would have a range from .1660 to .1412 with a mean of .1538.

The third series was made with a cast iron drum 3.03 inches in diameter. The surface of the drum smoothly turned but *not filed*. The cord was the same as used in the other two. Nine experiments were made at angles from 20° to 360°. The mean is .1753, the maximum .2133, and the minimum .1549.

For the fourth series the drum used in the third series was smoothly filed and observations made at the same angles as before. The mean this time is .1348, the maximum .1685, and the minimum .1089.

[The above experiments were made by Mr. C. G. Elliott in the Physical Laboratory of the Illinois Industrial University.]

THE VENTILATION OF THE MONT CENIS TUNNEL.

By WILLIAM POLE, F.R.SS. L. and E., M. Inst. C.E.

From Minutes of the Proceedings of the Institution of Civil Engineers.

In the discussion which took place at the Institution in January, 1876, on Mr. G. J. Morrison's Paper "On the ventilation and Working of Railway Tunnels," the Author mentioned, that on a visit to the Mont Cenis Tunnel in 1873, he saw some large exhausters at work at the north end, which he had reason to believe were used to effect an artificial ventilation of the tunnel. He explained however, that the information he obtained on that occasion was imperfect, and that it would be desirable to procure further data.

In the spring of 1877 he had an opportunity of again visiting the tunnel, and of obtaining the further particulars desired. The authorities of the Alta Italia railway in Turin, who have charge of the maintenance and working of the tunnel, courteously gave him all necessary facilities, and the engineer resident on the spot fully explained the works. He therefore thinks it right to make the necessary corrections and additions to his former statement, and so to put the Institution in possession of the true facts of the case.

The ventilation of the tunnel during its construction was described to the Institution by Mr. T. Sopwith, Jr., M. Inst. C.E., in his Paper of 1864; and in 1873 he added some further remarks about two years after the opening. It will be convenient therefore to take up the subject at the point where Mr. Sopwith left it.

No special works having reference to the permanent ventilation of the tunnel appear to have been included in the design. Mr. Sopwith stated there had been an expectation that, as the Italian end was 435 feet higher than the French end there would be a constant natural current established through the tunnel from north to south. But it is difficult to understand on what grounds such an expectation could have been based. It is true that the air at the southern entrance will, *cæteris paribus*, be more rarefied by about half an inch of mercury than that at the northern end; but as this rarefication is naturally due to the altitude it can have no effect in creating a current. In a pipe 435 feet long, placed vertically, the conditions would be similar, but they would cause no ascending current, as the air within the pipe would be in precisely the same condition as the external atmosphere around it. Hence the mere difference of level of the two ends of the tunnel, can, *per*

se, have no effect in producing ventilation.

This view was proved correct by experience, for, as Mr. Sopwith stated, no such current was found to exist, and the ventilation was often far from good. This evil was not of sufficient magnitude to annoy the passengers, but it was found "bad enough to render the work of the watchmen, rail-layers, and others employed in the tunnel insupportable at times."

To remedy this, advantage was taken of the air-compressing apparatus, which had been erected during the construction at Bardonnecchia, by laying a pipe about 8 inches diameter through the whole length of the tunnel, and placing cocks upon it at intervals of 125 méters. This pipe is still used; it is always kept supplied with air compressed to about six atmospheres, so that by opening any of these cocks a stream of fresh air, cooled by its expansion, is admitted at that part of the tunnel. Whenever, therefore, after the passage of a train, a man finds himself enveloped in a bad atmosphere, he opens the nearest air-cock and is at once relieved.

The Author saw this apparatus at work, and it appears to answer the partial and local purpose intended; but it is clear that it can do nothing worth speaking of to promote general ventilation, for the whole quantity of air supplied is only about 450 cubic feet per minute, a quantity much too small to produce any effective change.

It is also found that the loud hissing noise attending the escape of the air from the small apertures has, on some occasions, endangered the lives of the men by preventing them from hearing the approach of the trains.

At the north, or French end an additional arrangement is adopted, namely, the exhausting process to which the Author alluded in his former remarks. The origin of this arrangement was as follows:—The northern half of the tunnel inclines steeply upwards from its mouth, to the center of the mountain; and as the vitiated air generated during the works of construction would not naturally descend, it was necessary to extract it by force. For this object a channel or culvert was formed at the bottom of the tunnel, below formation level, and was carried along *pari passu* with the portion of the tunnel executed from the north end. Outside the tunnel on the hill above Modane were established large exhausting pumps, which, communicating with the channel, sucked, by its means, the vitiated air from the interior of the tunnel, the fresh air supplying its place, partly from the working of the compound air machines, but chiefly by entrance at the mouth, direct from the external atmosphere.

After the tunnel was completed, and when the defects of ventilation were felt, it was resolved to retain this apparatus in action, and it is at work still.

The exhausting apparatus consists of four large bell-vessels, like small gasholders, inverted in water. These are made to rise and fall by water-power, and being furnished with inlet and outlet valves, they act as air pumps, exhausting the air from a chamber below, which is in communication with the channel under the tunnel. Each bell is 5 méters in diameter, and works with a stroke of 2 meters, making six or eight strokes per minute. When the Author was there three of them were at work, at about six strokes per minute, and he calculated that they pumped in all nearly 25,000 cubic feet of air per minute.

The air-exhaust channel under the tunnel is of rectangular shape, 1 square mèter in area, and it has apertures at intervals of 500 meters, capable of being closed and opened at pleasure. Usually those nearest the mouth are closed, and the more distant ones open, so as to draw away the air as far in as possible; but the men open any of them when they find it necessary to clear a particular spot. Of course the fresh air enters from the mouth of the tunnel to supply the place of what is removed by the exhaustion. The Author inquired what was the practical effect of this exhausting process, and he was told that it was insufficient and unsatisfactory. The apertures nearest to the mouth were found to draw very well, but at a further distance away little or no draught was perceived, and consequently the process had no beneficial operation where it was wanted, that is, near the middle of the tunnel.

On examining the mechanical conditions of the problem, this disappointment is easily accounted for. The quan-

tity of air extracted gives a velocity along the exhaust-conduit of about 40 feet per second, and to overcome the frictional resistance due to this, over a length of several miles, would require much more power than the bell-pumps are able to afford. Their exhaustive force is only about 20 inches of water (100 lbs. per square foot), and they are incapable of doing more without exceeding their hydraulic seal. Hence, under the given conditions, the exhaustion can only act during the first mile or two of the tunnel, leaving all beyond unaffected.

From the foregoing description it may be inferred that the artificial processes of supplying compressed air at the Italian end, and of exhausting the air at the French end, although of some use locally and partially, can have no important influence in producing any thorough ventilation of the tunnel.

In the face, however, of this inference one is met with the undeniable fact that somehow or other, a considerable amount of general ventilation does go on. There must be a large quantity of vitiated air produced by the frequent passage of powerful engines, and yet it is not found that the passengers are incommoded thereby; on the contrary, they generally testify to the pleasantness of the atmosphere in passing through. Hence, although as before stated, inconvenience is found by the men immediately after the passage of trains, it is clear that a sufficient general movement must go on to effect, after a time, the entire removal of the noxious vapors. It will be interesting to inquire how this can be explained.

The mechanical action of the moving trains may be left out of the question; it would have no preceptible influence in moving the great mass of the contents of the tunnel, and in all probability the air only slips by them from the front to behind as they pass along.

It has been already remarked, too, that no current can be due to the mere difference of level of the two ends; but it may happen and no doubt does happen, that, independently of this, the barometric condition of the atmosphere generally may be different on the two sides of the Alps, and a very slight difference in this respect would suffice to create a powerful draught. Thus, if the air pressure, at the same altitude above sea-level, differs on the two sides of the mountain by only $\frac{1}{10}$ of an inch of mercury, this would suffice to create a current through the tunnel of $7\frac{1}{2}$ miles an hour. And there is no doubt that, from the very variable meteorological conditions in these high regions, such differences, or even much greater ones, must often occur. A difference of $\frac{1}{2}$ an inch of mercury would generate a current of 16 miles an hour.

In addition to this there is also the effect of the wind, as a brisk gale blowing from the north or the south, as the case may be, would have sufficient force to give rise to some current through. These meteorological conditions would no doubt vary much at different periods; sometimes they would act very powerfully, at other times they would not act at all. All this fully accounts for the the statment made by Mr. Sopwith from personal experience. He said "The difference in the rates of the air currents was very remarkable. During a few days he spent in the tunnel, on one day the air was almost stagnant, and on the following day he could hardly keep his hat on." This is just what might be expected from currents produced by meteorological changes.

There is, however, another cause of spontaneous ventilation which is always at work, with much more regularity, namely the *heating of the air inside the tunnel.* In regard to this, the difference of level of the two ends becomes a very material feature; the tunnel in fact assumes the function of a ventilating chimney 435 feet high; and when its contents are rarefied by heating, the production of an ascending current from north to south is perfectly natural. The heating of the air may occur in two ways: it may partly be caused by the higher temperature of the walls; for although the theories at first held as to the supposd high temperature of the interior of the mountain have not been borne out by experience, yet the heat is, no doubt, something greater than that outside. But the chief source of the heat will be the working of the engines; and it is matter of fact, that the general temperature inside the tunnel is maintained at a much higher degree than the external air at

the ends. According to Mr. Sopwith, this temperature may be estimated at 83° to 90° Fahr., which would give an elevation of from 30° to 60°; and calculation will show that, assuming all the air in the tunnel to be heated to this extent, it would suffice to establish a permanent current from Modane to Bardonnecchia.

The Author had, when he last came through the tunnel, from south to north, a practical proof of the existence of such a current; for although another train had shortly before gone up from Modane, filling the inclined part with smoke and vapor, yet as he approached the Modane end he found the atmosphere perfectly sweet and clear, the whole of the foulness having in the short interval been carried away.

These three causes, then, namely the difference in the barometer on the two sides, the wind, and the elevation of temperature inside the tunnel, appear in the aggregate to be effective at present in keeping up a fairly good spontaneous ventilation. The traffic, however, is not large, there being in all only twenty-two regular trains per day passing through. When this traffic is much increased, it may probably be necessary to do something to improve the ventilation by artificial means.

In reasoning from this case to others, it must be borne in mind that one of the causes of the present spontaneous ventilation (and probably the most active one) depends on the inclination of the tunnel. In such a case as that of the St. Gothard, which is nearly level, this cause cannot operate, and the Author is not aware what means are relied on for producing ventilation. The tunnel is a mile or two longer than that of Mont Cenis, and of course the difficulties will be proportionately increased.

THE DETERMINATION OF ROCKS—PORPHYRY.

By MELVILLE ATWOOD, F. G. S.

From "Journal of Microscopy."

The generally accepted meaning of the term porphyry, without addition or qualification, denotes "quartz porphyry," a plutonic rock, with a compact matrix or ground mass, consisting of quartz and feldspar, with crystals of both, having a specific gravity of from 2.5 to 2.6, and containing from 75 to 85 per cent. of silica.

What rock the Comstock miners mean when they say porphyry it would be exceedingly difficult for any one to tell. In reading over the published reports of the different Superintendents, they appear to be *continually* meeting with it at all depths, and to the east and west of the different bodies; also in the shape of "horse," or dead ground, mixed with the vein matter, and called "bird's eye porphyry." Now in ninety-nine cases out of the hundred, what they call porphyry does not in any one respect resemble that rock, lacking by 25 per cent the required amount of silica, and having no free quartz. A very slight examination by any one having only a rudimentary knowledge of geology would show that the term porphyry, so applied, is the most unappropriate that could be used to describe either the west or east country rock of the Virginia portion of the great Comstock Lode. The only way I can account for the use of that term is that they prefer it to saying "country rock."

I hardly think it requires me to say how desirable it would be, indeed, necessary, for those conducting explorations and trials on the Comstock Range, to know and be able to distinguish the different rocks they meet with in their operations, particularly as those rocks enclose some of the richest mines yet discovered in the world, and since the cost of those very explorations amount annually to millions and millions of dollars.

It may be urged that the geographical maps of the explorations of the fortieth parallel contain most of the necessary information. I would recommend those who think so to examine the maps, or any of the cross sections of the workings

on the Comstock, beautifully drawn—the work, I believe, of Mr. Stretch, but colored by the officers of the Survey, and they will find that the Mount Davidson diorite is colored as syenite, and the black dyke, a dolorite as andesite. Most of the country rocks overlaying the Comstock on the east are marked as propylite and andesite. The second propylite and andesite are identical in chemical and mineralogical composition, and a slight inspection of the Sutro Tunnel and other drainage levels will show that petrologically they are the same, in fact, the only difference being that the former occurs in sheets and the latter in dykes. When the feldspar in the so-called propylite is very much kaolinized, the rock is sometimes termed by the miners bird's eye porphyry. It may be said that the new work by Ferdinand Zirkel, "Microscopical Petrography," corrects most of those errors, by admitting in the first place that the Mount Davidson rock is a diorite. No mention, however, or section, is given of one of the most important rocks of the Comstock range, the black dyke; and in the explanation of the beautifully colored plates, he has neglected to state the number of times they are magnified. This is a most unfortunate omission, particularly so with respect to the basaltic rocks. My attention was called to it by some remarks in one of the Virginia papers, wherein it was stated that one of the handsome colored plates was a section of basalt from American Flat. Now, dolorite, anamesite and basalt, are, in a mineralogical point of view, the same rock, differing only in the fineness of texture.

The following are the approximate measurements of the crystals of feldspar in these rocks:

In the Black Dyke, they average from 7-600 to 20-600 in length, by 1-1200 to 10-1200 in width.

In the Dolerite, they are of irregular shape, but generally about double the size of those in the Black Dyke; while there are small masses containing small needle-shaped crystals 1-300 to 4-300 in length.

In the Basalt they average from 4-1200 to 9-1200 long, by 1-1200 to 2-1200 in width.

Eight pages, however, in that work, with a colored section, are devoted to what is called "Augite Andesite." Now though I have taken a great deal of trouble, as yet I have not been able to procure a single specimen of that rock called Andesite. A few months ago, a good authority on such matters, Alphons Stubel, of Dresden, passed through this city on his way home from South America, where he had been collecting rocks for many years. Knowing that he had been at Chimborazo, I thought it would be a good opportunity to get what I wanted; so when he called upon me I asked him as a favor to give me a specimen of andesite. He said that he was very sorry he could not comply with my request, that he really did not know any such rock.

I am fully aware that looseness in petrological nomenclature is the rule and not the exception, and that many geologists are found writing of totally different rocks under one and the same name. I do not think that any distinction between rocks is worth much unless it can be applied in the field. I have stated that the black dyke, a dolorite, but which from the fineness of its texture might be called anamesite, was one of the most important rocks in connection with the Comstock mines, from the fact that it forms the west boundary to all the vast treasures of the Comstock, no ore worth mentioning ever having been found at the west side of it; therefore every miner conducting operations in that district ought to possess the necessary amount of knowledge to enable him to distinguish that rock. If you will look at No. 12 rock and section, you will find it is fine-grained and apparently of so homogeneous a texture as not to admit of its constituent minerals being resolved by the naked eye. I have quite a collection of specimens which have been given to me, supposing them to be that rock.

In 1867, when engaged in the examination of the gold mines of North Wales, the well-known mining engineer, Mr. A. Dean, gave me the rough tracing of the working plans of the St. David's mine, Clogan, near Dolgelley, and which I have brought for your inspection. The geological features of that district are the Cambrian rocks, overlaid by the lower silurian. The St. David's vein is partly in the silurian slate beds, and sheets of

greenstone (diabase) lying between the slates, and partly in the Cambrians. What I particularly wish to draw your attention to, however, is the transverse section, showing the gold-bearing and non-gold-bearing rocks of the Clogan mines, and the very important fact that only those portions of the veins were rich in gold, or productive, where the walls were greenstone

Impressed with the truth of the discovery, on my return to California I devoted a large portion of my time to the examination of the enclosing and wall rocks of the gold and silver bearing veins of this Coast. On the formation of this Society, I availed myself of the aid of a microscope to carry on my investigations, but soon found out that to do so with anything like satisfactory result I must get a collection of well authenticated foreign types, to compare with and guide me in the work. Through the kindness of the late Mr. David Forbes, of London, Dr. Hector, of New Zealand, and, in San Fransisco, of Mr. H. G. Hanks and Mr. Charles Schneider, I have now a collection of some 500 specimens of foreign types, from which, with the assistance of my son, I have cut between 1,400 and 1,500 sections—some of them very roughly done. I found it necessary to have two or three from each specimen, some cut very thin and others rather thick, to show color and for examination with the aid of a parabolic illuminator. My collection of rock sections from this Coast is large; but the result of it all amounts to this; I found that every step I took I was traveling on a road that led me far away from what I wanted, which was, a method to make it easy for my fellow miners to understand and distinguish the enclosing and wall rocks of the different lodes they were working—these rocks having so much to do with the productivenes of the lodes.

By the merest chance, I have found out a simple way which I think, in a great measure, will partly fill the gap so much needed.

The different pieces of rock which I now present to the Society are roughly prepared after this method, and made so that an inspection of the outer surface viewed as an opaque object, with only the aid of a common hand-magnifier, will give all the information ordinarily required by the miner, and in most cases he will find that he is able to distinguish the structure and composition of all the commoner rocks, so that with the help of a small collection of foreign types, prepared after the same fashion, he can compare and identify those under examination. It will be necessary for them to read up a little on the subject, and to acquire a rudimentary knowledge of geology, which I think can be best done by a careful study of such works as "The Student's Manual of Geology," by J. Beete Jukes, 1857; "Text Book of Geology," by Dana; "A System of Mineralogy," by Dana; "A Treatise on Lithology," by Van Cotta, English Edition, by P. H. Lawrence; and "Determination of Rocks," by E. Jannettaz, translated by Plympton.

The rock for examination may be prepared as follows: First wash the specimen clean, using a brush to get rid of any clay and dirt; then select the side or part you wish to examine, and grind it down on a piece of sandstone (a shoemaker's sharpening stone) until a perfectly flat surface is obtained. This will occupy but a few minutes, unless the rock is very hard. The surface should then be worked down still finer with a square emery file, using water, and after you have obtained a sufficient polish, wash the rock again, and then let it dry gradually, either on a stove, or, what is better still, a little brass table, with a spirit lamp, the same that is used for heating slides. When perfectly dry, heat it again to a point, so that you can barely handle it; then polish the varnished side while hot with a mixture of one part of Canada balsam to three parts of alcohol, which must be warmed before applying it, and laid on with a camel's hair brush. It will soon dry, and if left for a day or two will harden, so that you can handle it without injury.

The effect of this treatment is remarkable, particularly on the lavas, as you will see by the specimen of trachyte lava from Bodie, which I now present to the Society.

In conclusion, it is with great hesitation that I have ventured to bring this matter before you, but I do so, well knowing that more searching and exact methods of investigation are now demanded by those conducting large mining opera-

tions, and that such terms as porphyry, for any and all enclosing, or wall rock, that may be met with in such mines as the Comstock, and the term green chlorides for the rich ore will not be deemed a sufficient explanation, or tend to give the mine adventurers that confidence in the reports of their employees which they should be entitled to, particularly when it is known that the rock is not porphyry, and that the chloride of silver is one of the accidental minerals met with in vein matter.

I am in hopes that by thus breaking the ice, others more capable in every respect than myself will be induced to communicate the results of their researches on the subject.

All that can be claimed for the mode I have suggested to you for the examination of rocks is that it is a rude and simple way of determining some of the commoner ones, but the application of the microscope, even now quite in its infancy, is, after all, what we must trust to for exact or reliable results.

MATHEMATICAL SCIENCE.

Abstract of the Address of Mr. WM. SPOTTISWOODE to the British Association.

From "The Engineer."

Although in its technical character mathematical science suffers the inconveniences, while it enjoys the dignity, of its Olympian position, still in a less formal garb, or in disguise, if you are pleased so to call it, it is found present at many an unexpected turn; and although some of us may never have learnt its special language, not a few have, all through our scientific life, and even in almost every accurate utterance, like Molière's well-known character, been talking mathematics without knowing it. It is, moreover, a fact not to be overlooked, that the appearance of isolation, so conspicuous in mathematics, appertains in a greater or less degree to all other sciences, and perhaps also to all pursuits in life. In its highest flight each soars to a distance from its fellows. Each is pursued alone for its own sake, and without reference to its connection with, or its application to, any other subject. The pioneer and the advanced guard are of necessity separated from the main body, and in this respect mathematics does not materially differ from its neighbors. And, therefore, as the solitariness of mathematics has been a frequent theme of discourse, it may be not altogether unprofitable to dwell for a short time upon the other side of the question, and to inquire whether there be not points of contact in method or in subject-matter between mathematics and the outer world which have been frequently overlooked; whether its lines do not in some cases run parallel to those of other occupations and purposes of life; and lastly, whether we may not hope for some change in the attitude too often assumed towards it by the representatives of other branches of knowledge and of mental activity. In his preface to the "Principia" Newton gives expression to some general ideas which may well serve as the key-note for all future utterances on the relation of mathematics to natural, including also therein what are commonly called artificial, phenomena. "The ancients divided mechanics into two parts, rational and practical; and since artisans often work inaccurately, it came to pass that mechanics and geometry were distinguished in this way, that everything accurate was referred to geometry, and everything inaccurate to mechanics. But the inaccuracies appertain to the artisan and not to the art, and geometry itself has its foundation in mechanical practice, and is in fact nothing else than that part of universal mechanics which accurately lays down and demonstrates the art of measuring." He next explains that rational mechanics is the science of motion resulting from forces, and adds: "The whole difficulty of philosophy seems to me to lie in investigating the forces of nature from the phenomena of

motion, and in demonstrating that from these forces other phenomena will ensue." Then, after stating the problems of which he has treated in the work itself, he says, "I would that all other natural phenomena might similarly be deduced from mechanical principles. For many things move me to suspect that everything depends upon certain forces in virtue of which the particles of bodies, through forces not yet understood, are either impelled together so as to cohere in regular figures, or are repelled and recede from one another." Newton's views, then, are clear. He regards mathematics, not as a method independent of, though applicable to, various subjects, but is itself the higher side or aspect of the subjects themselves; and it would be little more than a translation of his notions into other language, little more than a paraphrase of his own words, if we were to describe the mathematical as one aspect of the material world itself, apart from which all other aspects are but incomplete sketches, and however accurate after their own kind, are still liable to the imperfections of the inaccurate artificer. Mr. Burrowes, in his Preface to the first volume of the "Transactions of the Royal Irish Academy," has carried out the same argument, approaching it from the other side. "No one science," he says, "is so little connected with the rest as not to afford many principles whose use may extend considerably beyond the science to which they primarily belong, and no proposition is so purely theoretical as to be incapable of being applied to practical purposes. There is no apparent connection between duration and the cycloidal arch, the properties of which have furnished us with the best method of measuring time; and he who has made himself master of the nature and affections of the logarithmic curve has advanced considerably towards ascertaining the proportionable density of the air at various distances from the earth. The researches of the mathematician are the only sure ground on which we can reason from experiments; and how far experimental science may assist commercial interests is evinced by the success of manufacturers in countries where the hand of the artificer has taken its direction from the philosopher. Every manufacture is in reality but a chemical process, and the machinery requisite for carrying it on but the right application of certain propositions in rational mechanics." So far your academician. Every subject, therefore, whether in its usual acceptation, scientific, or otherwise, may have a mathematical aspect; as soon, in fact, as it becomes a matter of strict measurement, or of numerical statement, so soon does it enter upon a mathematical phase. This phase may, or it may not, be a prelude to another in which the laws of the subject are expreessed in algebraical formulæ or represented by geometrical figures. But the real gist of the business does not always lie in the mode of expression, and the fascination of the formulæ or other mathematical paraphernalia may after all be little more than that of a theatrical transformation scene. The process of reducing to formulæ is really one of abstraction, the results of which are not always wholly on the side of gain; in fact, through the process itself the subject may lose in one respect even more than it gains in another. But long before such abstraction is completely attained, and even in cases where it is never attained at all, a subject may to all intents and purposes become mathematical. It is not so much elaborate calculations or abstruse processes which characterize this phase as the principles of precision, of exactness, and of proportion. But these are principles with which no true knowledge can entirely dispense. If it be the general scientific spirit which at the outset moves upon the face of the waters, and out of the unknown depth brings forth light and living forms, it is no less the mathematical spirit which breathes the breath of life into what would otherwise have ever remained mere dry bones of fact, which re-unites the scattered limbs and re-creates from them a new and organic whole. And as a matter of fact, in the words used by Professor Jellett at our meeting at Belfast, viz., "Not only are we applying our methods to many sciences already recognized as belonging to the legitimate province of mathematics, but we are learning to apply the same instrument to sciences hitherto wholly or partially independent of its authority. Physical science is learning

more and more every day to see in the phenomena of nature modifications of that one phenomenon—namely, motion—which is peculiarly under the power of mathematics." Echoes are these, far off and faint perhaps, but still true echoes, in answer to Newton's wish that all these phenomena may some day "be deduced from mechanical principles." If turning from this aspect of the subject, it were my purpose to enumerate how the same tendency has evinced itself in the arts, unconsciously it may be to the artists themselves, I might call as witnesses each one in turn with full reliance on the testimony which they would bear. And, having more special reference to mathematics, I might confidently point to the accuracy of measurement, to the truth of curve, which, according to modern investigation, is the key to the perfection of classic art. I might triumphantly cite not only the architects of all ages, whose art so manifestly rests upon mathematical principles; but I might cite also the literary as well as the artistic remains of the great artists of Cinquecento, both painters and sculptors, in evidence of the geometry and the mechanics which, having been laid at the foundation, appear to have found their way upwards through the superstructure of their works. And in a less ambitious sphere, but nearer to ourselves in both time and place, I might point with satisfaction to the great school of English constructors of the eighteenth century in the domestic arts; and remind you that not only the engineer and the architect, but even the cabinetmakers devoted half the space of their books to perspective and to the principles whereby solid figures may be delineated on paper, or what is now termed descriptive geometry. Nor perhaps would the sciences which concern themselves with reasoning and speech, nor the kindred art of music, nor even literature itself, if thoroughly probed, offer fewer points of dependence upon the science of which I am speaking. What, in fact, is logic but that part of universal reasoning; grammar but that part of universal speech; harmony and counterpoint but that part of universal music; "which accurately lays down," and demonstrates—so far as demonstration is possible—precise methods appertaining to each of these arts? And I might even appeal to the common consent which speaks of the mathematical as the pattern form of reasoning and model of a precise style. Taking, then, precision and exactness as the characteristics which distinguish the mathematical phase of a subject, we are naturally led to expect that the approach to such a phase will be indicated by increasing application of the principle of measurement, and by the importance which is attached to numerical results. And this very necessary condition for progress may, I think, be fairly described as one of the main features of scientific advance in the present day. If it were my purpose, by descending into the arena of special sciences, to show how the most varied investigations alike tend to issue in measurement, and to that extent to assume a mathematical phase, I should be embarrassed by the abundance of instances which might be adduced. I will, therefore, confine myself to a passing notice of a very few, selecting those which exemplify not only the general tendency, but also the special character of the measurements now particularly required, viz., that of minuteness, and the indirect method by which alone we can at present hope to approach them. An object having a diameter of an 80,000th of an inch is perhaps the smallest of which the microscope could give any well-defined representation; and it is improbable that one of 120,000th of an inch could be singly discerned with the highest powers at our command. But the solar beams and the electric light reveal to us the presence of bodies far smaller than these. And, in the absence of any means of observing them singly, Professor Tyndall has suggested a scale of these minute objects in terms of the lengths of luminiferous waves. To this he was led, not by any attempt at individual measurement, but by taking account of them in the aggregate, and observing the tints which they scatter laterally when clustered in the form of actinic clouds. The small bodies with which experimental science has recently come into contact are not confined to gaseous molecules, but comprise also complete organisms; and the same philosopher has made a profound study of the momentous influence exerted by these

minute organisms in the economy of life. And if, in view of their specific effects, whether deleterious or other, on human life, any qualitative classification, or quantitative estimate be ever possible, it seems that it must be effected by some such method as that indicated above. Again, to enumerate a few more instances of the measurement of minute quantities, there are the average distances of molecules from one another in various gases and at various pressures; the length of their free path, or range open for their motion without coming into collision; there are movements causing the pressures and differences of pressure under which Mr. Crookes' radiometers execute their wonderful revolutions. There are the excursions of the air while transmitting notes of high pitch, which through the researches of Lord Rayleigh appear to be of a diminutiveness altogether unexpected. There are the molecular actions brought into play in the remarkable experiments by Dr. Kerr, who has succeeded, where even Faraday failed, in effecting a visible rotation of the plane of polarisation of light in its passage through electrified dielectrics, and on its reflexion at the surface of a magnet. To take one more instance, which must be present to the minds of us all, there are the infinitesimal ripples of the vibrating plate in Mr. Graham Bell's most marvelous invention. Of the nodes and ventral segments in the plate of the telephone which actually convert sound into electricity and electricity into sound, we can at present form no conception. All that can now be said is that the most perfect specimens of Chladni's sand figures on a vibrating plate, or of Kundt's lycopodium heaps in a musical tube, or even Mr. Sedley Taylor's more delicate voitices in the films of the phoneidoscope, are rough and sketchy compared with these. For notwithstanding the fact that in the movement of the telephone plate we have actually in our hand the solution of that old world problem, the construction of a speaking machine; yet the characters in which that solution is expressed are too small for our powers of decipherment. In movements such as these we seem to lose sight of the distinction, or perhaps we have unconsciously passed the boundary between massive and molecular motion. Through the phonograph we have not only a transformation but a permanent and tangible record of the mechanism of speech. But the differences upon which articulation (apart from loudness, pitch, and quality) depends, appear from the experiments of Fleemin Jenkin and of others to be of microscopic size. The microphone affords another instance of the unexpected value of minute variations—in this case of electric currents; and it is remarkable that the gist of the instrument seems to lie in obtaining and perfecting that which electricians have hitherto most scrupulously avoided, viz., loose contact. Once more, Mr. De La Rue has brought forward as one of the results derived from his stupendous battery of 10,000 cells, strong evidence for supposing that a voltaic discharge, even when apparently continuous, may still be an intermittent phenomenon; but all that is known of the period of such intermittence is, that it must recur at exceedingly short intervals. And in connection with this subject, it may be added that, whatever be the ultimate explanation of the strange stratification which the voltaic discharge undergoes in rarefied gases, it is clear that the alternate disposition of light and darkness must be dependent on some periodic distribution in space or sequence in time which can at present be dealt with only in a very general way. In the exhausted column we have a vehicle for electricity not constant like an ordinary conductor, but itself modified by the passage of the discharge, and perhaps subject to laws differing materially from those which it obeys at atmospheric pressure. It may also be that some of the features accompanying stratification from a magnified image of phenomena belonging to disruptive discharges in general; and that consequently, so far from expecting among the known facts of the latter any clue to an explanation of the former, we must hope ultimately to find in the former an elucidation of what is at present obscure in the latter. A prudent philosopher usually avoids hazarding any forecast of the practical application of a purely scientific research. But it would seem that the configuration of these striæ might some day prove a very delicate means of estimating low pressures. Now,

it is a curious fact that almost the only small quantities of which we have as yet any actual measurements are the wave lengths of light; and that all others, excepting so far as they can be deduced from these, await future determination. In the meantime, when unable to approach these small quantities individually, the method to which we are obliged to have recourse is, as indicated above, that of averages, whereby, disregarding the circumstances of each particular case, we calculate the average size, the average velocity, the average direction, &c., of a large number of instances. But although this method is based upon experience, and leads to results which may be accepted as substantially true; although it may be applicable to any finite interval of time, or over any finite area of space (that is, for all practical purposes of life), there is no evidence to show that it is so when the dimensions of interval or of area are indefinitely diminished. The truth is that the simplicity of nature which we at present grasp is really the result of infinite complexity; and that below the uniformity there underlies a diversity whose depths we have not yet probed, and whose secret places are still beyond our reach. The present is not an occasion for multiplying illustrations, but I can hardly omit a passing allusion to one all-important instance of the application of the statistical method. Without its aid social life, or the history of life and death, could not be conceived at all, or only in the most superficial manner. Without it we could never attain to any clear ideas of the condition of the poor, we could never hope for any solid amelioration of their condition or prospects Without its aid, sanitary measures, and even medicine would be powerless. Without it, the politician and the philanthropist would alike be wandering over a trackless desert. It is, however, not so much from the side of science at large as from that of mathematics itself, that I desire to speak. I wish from the latter point of view to indicate connections between mathematics and other subjects, to prove that hers is not after all such a far-off region, nor so undecipherable an alphabet, and to show that even at unlikely spots we may trace under-currents of thought which having issued from a common source fertilise alike the mathematical and the non-mathematical world. Having this in view, I propose to make the subject of special remark some process peculiar to modern mathematics; and, partly with the object of incidentally removing some current misapprehensions, I have selected for examination three methods in respect of which mathematicians are often thought to have exceeded all reasonable limits of speculation, and to have adopted for unknown purposes an unknown tongue. And it will be my endeavor to show not only that in these very cases our science has not outstepped its own legitimate range, but that even art and literature have unconsciously employed methods similar in principle. The three methods in question are, first, that of imaginary quantities; secondly, that of manifold space; and thirdly, that of geometry not according to Euclid. First it is objected that, abandoning the more cautious methods of ancient mathematicians, we have admitted into our formulæ quantities which by our own showing, and even in our own nomenclature, are imaginary or impossible; nay, more, that out of them we have formed a variety of new algebras to which there is no counterpart whatever in reality, but from which we claim to arrive at possible and certain results. On this head it is in Dublin, if anywhere, that I may be permitted to speak. For to the fertile imagination of the late Astronomer Royal for Ireland we are indebted for that marvellous Calculus of Quaternions, which is only now beginning to be fully understood, and which has not yet received all the applications of which it is doubtless capable. And even although this calculus be not co-extensive with another which almost simultaneously germinated on the Continent, nor with ideas more recently developed in America, yet it must always hold its position as an original discovery, and as a representative of one of the two great groups of generalized algebras—viz., those the squares of whose units are respectively negative, unity and zero—the common origin of which must still be marked on our intellectual map as an unknown region. Well do I recollect how in its early days we used to handle the method as a magician's page might

try to wield his master's wand, trembling as it were between hope and fear, and hardly knowing whether to trust our own results until they had been submitted to the present and ever-ready counsel of Sir W. R. Hamilton himself. To fix our ideas, consider the measurement of a line, or the reckoning of time, or the performance of any mathematical operation. A line may be measured in one direction or in the opposite; time may be reckoned forward or backward; an operation may be performed or be reversed, it may be done or may be undone; and if having once reversed any of these processes we reverse it a second time, we shall find that we have come back to the original direction of measurement or of reckoning, or to the original kind of operation. Suppose, however, that at some stage of a calculation our formulæ indicate an alteration in the mode of measurement such that, if the alteration be repeated, a condition of things, not the same as, but the reverse of the original, will be produced. Or suppose that, at a certain stage, our transformations indicate that time is to be reckoned in some manner different from future or past, but still in a way having definite algebraical connection with time which is gone and time which is to come. It is clear that in actual experience there is no process to which such measurements correspond. Time has no meaning except as future or past; and the present is but the meeting point of the two. Or, once more, suppose that we are gravely told that all circles pass through the same two imaginary points at an infinite distance, and that every line drawn through one of these points is perpendicular to itself. On hearing the statement, we shall probably whisper, with a smile or a sigh, that we hope it is not true; but that in any case it is a long way off, and perhaps, after all, it does not very much signify. If, however, as mathematicians we are not satisfied to dismiss the question on these terms, we ourselves must admit that we have here reached a definite point of issue. Our science must either give a rational account of the dilemma, or yield the position as no longer tenable. Special modes of explaining this anomalous state of things have occurred to mathematicians. But, omitting details as unsuited to the present occasion, it will, I think, be sufficient to point out in general terms that a solution of the difficulty is to be found in the fact that the formulæ which give rise to these results are more comprehensive than the signification assigned to them; and when we pass out of the condition of things first contemplated they cannot—as it is obvious they ought not—give us any results intelligible on that basis. But it does not therefore by any means follow that upon a more enlarged basis the formulæ are incapable of interpretation; on the contrary, the difficulty at which we have arrived indicates that there must be some more comprehensive statement of the problem which will include cases impossible in the more limited, but possible in the wider view of the subject. A very simple instance will illustrate the matter. If from a point outside a circle we draw a straight line to touch the curve, the distance between the starting point and the point of contact has certain geometrical properties. If the starting point be shifted nearer and nearer to the circle the distance in question becomes shorter, and ultimately vanishes. But as soon as the point passes to the interior of the circle the notion of a tangent and distance to the point of contact cease to have any meaning; and the same anomalous condition of things prevail, as long as the point remains in the interior. But if the point be shifted still further until it emerges on the other side, the tangent and its properties resume their reality, and are as intelligible as before. Now the process whereby we have passed from the possible to the impossible, and again repassed to the possible (namely, the shifting of the starting point) is a perfectly continuous one, while the conditions of the problem as stated above have abruptly changed. If, however, we replace the idea of a line touching by that of a line cutting the circle, and the distance of the point of contact by the distances at which the line is intercepted by the curve, it will easily be seen that the latter includes the former as a limiting case, when the cutting line is turned about the starting point until it coincides with the tangent itself. And further, that the two intercepts have a perfectly distinct and intelligible meaning whether the point be outside or inside the area.

The only difference is that in the first case the intercepts are measured in the same direction; in the latter in opposite directions. The foregoing instance has shown one purpose which these imaginaries may serve, viz., as marks indicating a limit to a particular condition of things, to the application of a particular law, or pointing out a stage where a more comprehensive law is required. To attain to such a law we must, as in the instance of the circle and tangent, reconsider our statement of the problem; we must go back to the principle from which we set out, and ascertain whether it may not be modified or enlarged. And even if in any particular investigation, wherein imaginaries have occurred, the most comprehensive statement of the problem of which we are at present capable fails to give an actual representation of these quantities; if they must for the present be relegated to the category of imaginaries; it still does not follow that we may not at some future time find a law which will endow them with reality, nor that in the mean time we need hesitate to employ them, in accordance with the great principle of continuity, for bringing out correct results. If, moreover, both in geometry and in algebra we occasionally make use of points or of quantities, which from our present outlook have no real existence, which can neither be delineated in space of which we have experience, nor measured by scale as we count measurement; if these imaginaries, as they are termed, are called up by legitimate processes of our science; if they serve the purpose not merely of suggesting ideas, but of actually conducting us to practical conclusions; if all this be true in abstract science, I may perhaps be allowed to point out, in illustration of my argument, that in art unreal forms are frequently used for suggesting ideas, for conveying a meaning for which no others seem to be suitable or adequate. Are not forms unknown to biology, situations incompatible with gravitation, positions which challenge not merely the stability but even the possibility of equilibrium—are not these the very means to which the artist often has recourse in order to convey his meaning and to fulfill his mission? Who that has ever revelled in the ornamentation of the Renaissance, in the extraordinary transitions from the animal to the vegetable, from faunic to floral forms, and from these again to almost purely geometric curves, who has not felt that these imaginaries have a claim to recognition very similar to that of their congeners in mathematics? How is it that the grotesque paintings of the middle ages, the fantastic sculpture of remote nations, and even the rude art of the pre-historic past, still impress us, and have an interest over and above their antiquarian value; unless it be that they are symbols which, although hard of interpretation when taken alone, are yet capable, from a more comprehensive point of view, of leading us mentally to something beyond themselves, and to truths which, although reached through them, have a reality scarcely to be attributed to their outward forms? Again, if we turn from art to letters, truth to nature and to fact is undoubtedly a characteristic of sterling literature; and yet in the delineation of outward nature itself, still more in that of feelings and affections, of the secret parts of character and motives of conduct, it frequently happens that the writer is driven to imagery, to an analogy, or even to a paradox, in order to give utterance to that of which there is no direct counterpart in recognized speech. And yet which of us cannot find a meaning for these literary figures, an inward response to imaginative poetry, to social fiction, or even to those tales of giant and fairy-land, written, it is supposed, only for the nursery or schoolroom? But in order thus to reanimate these things with a meaning beyond that of the mere words, have we not to reconsider our first position, to enlarge the ideas with which we started; have we not to cast about for something which is common to the idea conveyed and to the subject actually described, and to seek for the sympathetic spring which underlies both; have we not, like the mathematician, to go back as it were to some first principles, or, as it is pleasanter to describe it, to become again as a little child? Passing to the second of the three methods, viz., that of manifold space, it may first be remarked that our whole experience of space is in three dimensions, viz., of that which has length, breadth, and thickness; and if for certain purposes we restrict our ideas

to two dimensions as in plane geometry, or to one dimension as in the division of a straight line, we do this only by consciously and of deliberate purpose setting aside, but not annhilating, the remaining one or two dimensions. Negation, as Hegel has justly remarked, implies that which is negatived, or as he expresses it, affirms the opposite. It is by abstraction from previous experience, by a limination of its results, and not by independent process, that we arrive at the idea of space whose dimensions are less than three. It is doubtless on this account that problems in plane geometry which, although capable of solution on their own account, become much more intelligible, more easy of extension, if viewed in connection with solid space, and as special cases of corresponding problems in solid geometry. So eminently is this the case, that the very language of the more general method often leads us almost intuitively to conclusions which, from the more restricted point of view, require long and laborious proof. Such a change in the base of operations has, in fact, been successfully made in geometry of two dimensions, and although we have not the same experimental data for further steps, yet neither the modes of reasoning, nor the validity of its conclusions, are in any way affected by applying an analogous mental process to geometry of three dimensions; and by regarding figures in space of three dimensions as sections of figures in space of four, in the same way that figures in plano are sometimes considered as sections of figures in solid space. The addition of a fourth dimension to space not only extends the actual properties of geometrical figures, but it also adds new properties which are often useful for the purposes of transformation or of proof. Thus it has recently been shown that in four dimensions a closed material shell could be turned inside out by simple flexure, without either stretching or tearing; and that in such a space it is impossible to tie a knot. Again, the solution of problems in geometry is often effected by means of algebra; and as three measurements, or co-ordinates as they are called, determine the position of a point in space, so do three letters or measurable quantities serve for the same purpose in the language of algebra. Now, many algebraical problems involving three unknown or variable quantities admit of being generalized so as to give problems involving many such quantities. And as, on the other hand, to every algebraical problem involving unknown quantities or variables by ones, or by twos, or by threes, there corresponds a problem in geometry of one or of two or of three dimensions; so on the other it may be said that to every algebraical problem involving many variables there corresponds a problem in geometry of many dimensions. There is, however, another aspect under which even ordinary space presents to us a four-fold, or indeed a mani-fold character. In modern physics, space is regarded not as a vacuum in which bodies are placed and forces have play, but rather as a plenum with which matter is co-extensive. And, from a physical point of view, the properties of space are the properties of matter, or of the medium which fills it. Similarly from a mathematical point of view, space may be regarded as a *locus in quo*, as a plenum, filled with those elements of geometrical magnitude which we take as fundamental. These elements need not always be the same. For different purposes different elements may be chosen; and upon the degree of complexity of the subject of our choice will depend the internal structure or manifoldness of space. Thus beginning with the simplest case, a point may have any singly infinite multitude of positions in a line, which gives a one-fold system of points in a line. The line may revolve in a plane about any one of its points, giving a two-fold system of points in a plane; and the plane may revolve about any one of the lines, giving a three-fold system of points in space. Suppose, however, that we take a straight line as our element, and conceive space as filled with such lines. This will be the case if we take two planes, *e.g.*, two parallel planes, and join every point in one with every point in the other. Now the points in a plane form a two-fold system, and it therefore follows that the system of lines is four-fold; in other words, space regarded as a plenum of lines is four-fold. The same result follows from the consideration that the lines in a plane, and the planes through a point are each two-fold.

Again, if we take a sphere as our element we can through any point as a center draw a singly infinite number of spheres, but the number of such centers is triply infinite; hence space as a plenum of spheres is four-fold. And, generally, space as a plenum of surfaces has a manifoldness equal to the number of constants required to determine the surface. Although it would be beyond our present purpose to attempt to pursue the subject further, it should not pass unnoticed that the identity in the four-fold character of space, as derived on the one hand from a system of straight lines, and on the other from a system of spheres, is intimately connected with the principles established by Sophus Lie in his researches on the correlation of these figures. If we take a circle as our element we can around any point in a plane as a center draw a singly infinite system of circles; but the number of such centers in a plane is doubly infinite; hence the circles in a plane form a three-fold system, and as the planes in space form a three-fold system, it follows that space as a plenum of circles is six-fold. Again, if we take a circle as our element, we may regard it as a section either of a sphere, or of a right cone—given except in position—by a plane perpendicular to the axis. In the former case the position of the center is three-fold; the directions of the plane, like that of a pencil of lines perpendicular thereto, two-fold; and the radius of the sphere one-fold; six-fold in all. In the latter case, the position of the vertex is three-fold; the direction of the axis two-fold; and the distance of the plane of section one-fold; six-fold in all, as before. Hence space as a plenum of circles is six-fold. Similarly, if we take a conic as our element we may regard it as a section of a right cone—given except in position—by a plane. If the nature of the conic be defined, the plane of section will be inclined at a fixed angle to the axis; otherwise it will be free to take any inclination whatever. This being so, the position of the vertex will be three-fold; the direction of the axis two-fold; the distance of the plane of section from the vertex one-fold; and the direction of that plane one-fold if the conic be defined, two-fold if it be not defined. Hence, space as a plenum of definite conics will be seven-fold, as a plenum of conics in general eight-fold. And so on for curves of higher degrees. This is, in fact, the whole story and mystery of manifold space. It is not seriously regarded as a reality in the same sense as ordinary space; it is a mode of representation, or a method which, having served its purpose, vanishes from the scene. Like a rainbow, if we try to grasp it, it eludes our very touch; but like a rainbow, it arises out of real conditions of known and tangible quantities, and if rightly apprehended it is a true and valuable expression of natural laws, and serves a definite purpose in the science of which it forms a part.

The third method proposed for special remark is that which has been termed Non-Euclidean Geometry; and the train of reasoning which has led to it may be described in general terms as follows: some of the properties of space which on account of their simplicity, theoretical as well as practical, have, in constructing the ordinary system of geometry, been considered as fundamental, are now seen to be particular cases of more general properties. Thus a plane surface, and a straight line, may be regarded as special instances of surfaces and lines whose curvature is everywhere uniform or constant. And it is perhaps not difficult to see that, when the special notions of flatness and straightness are abandoned, many properties of geometrical figures which we are in the habit of regarding as fundamental will undergo profound modification. Thus a plane may be considered as a special case of the sphere, viz., the limit to which a sphere approaches when its radius is increased without limit. But even this consideration trenches upon an elementary proposition relating to one of the simplest of geometrical figures. In plane triangles the interior angles are together equal to two right angles; but in triangles traced on the surface of a sphere this proposition does not hold good. To this, other instances might be added.

It has often been asked whether modern research in the field of pure mathematics has not so completely outstripped its physical applications as to be practically useless; whether the analyst and the geometer might not now, and for a long time to come, fairly say,

"*Hic artem remumque repono*," and turn his attention to mechanics and to physics. That the pure has outstripped the applied is largely true; but that the former is on that account useless is far from true. Its utility often crops up at unexpected points; witness the aids to classification of physical quantities, furnished by the ideas—of Scalar and Vector—involved in the calculus of Quaternions; or the advantages which have accrued to physical astronomy from Lagrange's equations, and from Hamilton's principle of varying action; on the value of complex quantities, and the properties of general integrals, and of general theorems on integration for the theories of electricity and magnetism. The utility of such researches can in no case be discounted, or even imagined beforehand; who, for instance, would have supposed that the calculus of forms or the theory of substitutions would have thrown much light upon ordinary equations; or that abelian functions and hyperelliptic transcendents would have told us anything about the properties of curves; or that the calculus of operations would have helped us in any way towards the figure of the earth? But upon such technical points I must not dwell. If, however, as I hope, it has been sufficiently shown that any of these more extended ideas enable us to combine together, and to deal with as one, properties and processes which from the ordinary point of view present marked distinctions, then they will have justified their own existence; and in using them we shall not have been walking in a vain shadow, nor disquieting our brains in vain. These extensions of mathematical ideas would, however, be overwhelming, if they were not compensated by some simplifications in the processes actually employed. Of these aids to calculation I will mention only two, viz., symmetry of form, and mechanical appliances; or, say, mathematics as a fine art, and mathematics as a handicraft. And first, as to symmetry of form. There are many passages of algebra in which long processes of calculation at the outset seem unavoidable. Results are often obtained in the first instance through a tangled maze of formulæ, where at best we can just make sure of our progress step by step, without any general survey of the path which we have traversed, and still less of that which we have to pursue. But almost within our own generation a new method has been devised to clear this entanglement. More correctly speaking, the method is not new, for it is inherent in the processes of algebra itself, and instances of it, unnoticed perhaps or disregarded, are to be found cropping up throughout nearly all mathematical treatises. By Lagrange, and to some extent also by Gauss, among the older writers, the method of which I am speaking was recognized as a principle; but beside these, perhaps, no others can be named until a period within our own recollection. The method consists in symmetry of expression. In algebraical formulæ combinations of the quantities entering therein occur and recur; and by a suitable choice of these quantities the various combinations may be rendered symmetrical, and reduced to a few well-known types. This having been done, and one such combination having been calculated, the remainder, together with many of their results, can often be written down at once, without further calculations, by simple permutations of the letters. Symmetrical expressions, moreover, save as much time and trouble in reading as in writing. Instead of wading laboriously through a series of expressions which, although successively dependent, bear no outward resemblance to one another, we may read off symmetrical formulæ, of almost any length, at a glance. A page of such formulæ becomes a picture; known forms are seen in definite groupings; their relative positions, or perspective as it may be called, their very light and shadow, convey their meaning almost as much through the artistic faculty as through any conscious ratiocinative process. Few principles have been more suggestive of extended ideas or of new views and relations than that of which I am now speaking. In order to pass from questions concerning plane figures to those which appertain to space, from conditions having few degrees of freedom to others which have many—in a word, from more restricted to less restricted problems—we have in many cases merely to add lines and columns to our array of letters or symbols already formed, and then read off pictorially the

extended theorems. Next as to mechanical appliances. Mr. Babbage, when speaking of the difficulty of insuring accuracy in the long numerical calculations of theoretical astronomy, remarked, that the science which in itself is the most accurate and certain of all, had through those difficulties become inaccurate and uncertain in some of its results. And it was doubtless some such consideration as this, coupled with his dislike of employing skilled labor where unskilled would suffice, which led him to the invention of his calculating machines. The idea of substituting mechanical for intellectual power has not lain dormant; for beside the arithmetical machines whose name is legion—from Napier's Bones, Earl Stanhope's calculator, to Schultz and Thomas's machines now in actual use—an invention has lately been designed for even a more difficult task. Prof. James Thomson has in fact recently constructed a machine which, by means of the mere friction of a disc, a cylinder, and a ball, is capable of effecting a variety of the complicated calculations which occur in the highest application of mathematics to physical problems. By its aid it seems that an unskilled laborer may, in a given time, perform the work of ten skilled arithmeticians. The machine is applicable alike to the calculation of tidal, of magnetic, of meteorological, and perhaps also of all other periodic phenomena. It will solve differential equations of the second and perhaps of even higher orders. And through the same invention the problem of finding the free motions of any number of mutually attracting particles, unrestricted by any of the approximate suppositions required in the treatment of the lunar and planetary theories, is reduced to the simple process of turning a handle.

Coterminous with space and coeval with time is the kingdom of mathematics; within this range her dominion is surpreme; otherwise than according to her order nothing can exist; in contradiction to her laws nothing takes place. On her mysterious scroll is to be found written for those who can read it that which has been, that which is, and that which is to come. Everything material which is the subject of knowledge has number, order, or position; and these are her first outlines for a sketch of the universe. If our more feeble hands cannot follow out the details, still her part has been drawn with an unerring pen, and her work cannot be gainsayed. So wide is the range of mathematical science, so indefinitely may it extend beyond our actual powers of manipulation, that at some moments we are inclined to fall down with even more than reverence before her majestic presence. But so strictly limited are her promises and powers, about so much that we might wish to know does she offer no information whatever, that at other moments we are fain to call her results but a vain thing, and to reject them as a stone when we had asked for bread. If one aspect of the subject encourages our hopes, so does the other tend to chasten our desires; and he is perhaps the wisest and, in the long run, the happiest among his fellows who has learnt not only this science, but also the larger lesson which it indirectly teaches, namely, to temper our aspirations to that which is possible, to moderate our desires to that which is attainable, to restrict our hopes to that of which accomplishment, if not immediately practicable, is at least distinctly within the range of conception. That which at present is beyond our ken may, at some period and in some manner as yet unknown to us fall within our grasp; but our science teaches us, while ever yearning with Goethe for "Light, more light," to concentrate our attention upon that of which our powers are capable, and contentedly to leave for future experience the solution of problems to which we can at present say neither yea nor nay. It is within the region thus indicated that knowledge in the true sense of the word is to be sought. Other modes of influence there are in society and in individual life, other forms of energy besides that of intellect. There is the potential energy of sympathy, the actual energy of work; there are the vicissitudes of life, the diversity of circumstance, health and disease, and all the perplexing issues, whether for good or for evil, of impulse and of passion. But although the book of life cannot at present be read by the light of science alone, nor the wayfarers be satisfied by the few loaves of knowledge now in our hands; yet it would be difficult to over-

state the almost miraculous increase which may be produced by a liberal distribution of what we already have, and by a restriction of our cravings within the limits of possibility. In proportion as method is better than impulse, deliberate purpose than erratic action, the clear glow of sunshine than irregular reflection, and definite utterances than an uncertain sound; in proportion as knowledge is better than surmise, proof than opinion; in that proportion will the mathematician value a discrimination between the certain and the uncertain, and a just estimate of the issues which depend upon one motive power or the other. While on the one hand he accords to his neighbors full liberty to regard the unknown in whatever way they are led by the noblest powers that they possess; so on the other he claims an equal right to draw a clear line of demarcation between that which is a matter of knowledge, and that which is at all events something else, and to treat the one category as fairly claiming our assent, the other as open to further evidence.

And yet, when he sees around him those whose aspirations are so fair, whose impulses so strong, whose receptive faculties so sensitive, as to give objective reality to what is often but a reflex from themselves, or a projected image of their own experience, he will be willing to admit that there are influences which he cannot as yet either fathom or measure, but whose operation he must recognize among the facts of our existence.

THE MAGNETIC NEEDLE—THE CAUSE OF ITS SECULAR VARIATIONS.

By THOMAS JOB, Utah.

VARIATION IN THE DECLINATION.

Nearly three centuries ago philosophers observed that the magnetic needle did not always lie in the same direct line, even on the same meridian, but that in the northern hemisphere its north pole has a secular movement around a certain point or pole, not far from the pole of the world; it points sometimes to the east and at other times to the west of the same meridian, performing the northern half of a revolution in 318 years. "The Earth a Great Magnet" (Prof. A. M. Mayer.) A very remarkable phenomenon is observed—it follows the law of a swinging pendulum—retarding in velocity from the meridian of the station to its easterly or westerly tropic.

In the year 1622 the declination of the needle at London was 6° to the east of the geographical meridian. In 1660 the needle pointed due north and south, thus varying 6° in 38 years, while vibrating near the meridian of the place. In 1818 the needle varied, according to Prof. Watts, 24° 36′ to the west, and in 1865, 21° 6′ west; that is, varying only 3° 35′ in 45 years, when moving near its westerly tropic.

The cause of this secular change in the declination of the compass needle has been a theme of investigation with philosophers ever since its discovery, and in no time more ardently than in our day; but no satisfaction has yet been given to scientists. All that has been accomplished by observers is to show that the north magnetic pole is now vibrating from west to east, and at London, approaching the meridian.

It has been further observed that the magnetic needle, in its grand secular swing, makes some minor vibrations and deflections, some of which appear to follow regular laws and be periodical; their physical cause is found to be dependent on the sun as primary mover; others are evidently irregular changes, disturbing more or less the periodical variations.

The most remarkable of the periodical variations is what is called the daily vibration; it manifests its relation to the sun by following him in his apparent daily motion around the earth, in the northern hemisphere, and during the hours of the day from east to west, and from west to east in the hours of the

night; but the contrary way in the southern hemisphere.

These easterly and westerly variations in all parts of the globe where observations have been made, are obviously governed by distinct laws. The westerly deflections in the British Isles, as represented by the self-moving records at Kew, as Dr. Noades observes, have their chief prevalence from 5 A. M. to 5 P. M., and the easterly deflections during the remaining hours, causing the needle to return to its former position by 5 o'clock the next morning.

The extent of the daily oscillation of the needle is small, and also variable. Its mean value at Philadelphia, as observed by Dr. Bache, is 7.5′. The mean extent of the vibration at any station varies with the daily changes in the sun's declination, and so having semi-annual inequality, being deflected towards the east, and therefore with a negative sign, or less than unity, when the sun is north of the equator; but toward the west, and consequently more than the mean, when the sun is south of the equator.

The annual variation, independent of the daily, is a very small quantity, amounting, in the British Isles, to only about 59.56 sec., as given by General Sabine, being 28.95 sec., from March 21st to the 21st of September, with the signs minus and plus 29.9 sec., during the remaining six months. It affects in like manner both the northern and southern needles.

The daily variation of the needle also varies with variation in the latitude of the observer; reckoning from a certain, and seemingly fixed line, termed the *magnetic equator*. In fact the needle, in its daily swing, does not play backward and forward, pendulum-like, across the meridian of a station, but virtually its north pole revolves with the sun around the earth—toward the west in the northmen's day, and toward the east in the day of the southern hemisphere. So in the southern hemisphere the motion of the needle appears to be reversed, towards the east in the day time and towards the west in the night.

The case is also the same with the *secular vibration;* in the southern hemisphere the needle appears to vibrate in the opposite direction to what it does in the northern.

Only that part of the daily motion in which the needle swings westward belongs to the northern hemisphere; the same with its corresponding secular vibration; and that part below the earth, where the needle moves from west to east, represents the secular swing in the southern hemisphere; even as it is day there when it is night with us, and the positive pole of the needle follows the sun.

Proper investigation will show that this daily vibration is the fundamental cause of both the annular and the secular variations of the magnetic needle.

There are in our common year 366 siderial days, but only about 365¼ solar days, that is, while the earth rotates 366 times on its axis it revolves once in an orbit around the sun in the same direction,—from west to east,—and thus we have only 365¼ days out of 366 earth rotations; so the sun appears as if to step backwards—toward the west—from the earth, to the amount of one day's motion in a year. Thus he continues to recede westward from the earth—in the northern hemisphere, by the same space, year after year, till he returns again to the starting point in the orbit, where the earth will meet him, after gaining on him one whole revolution. The pole of the magnetic needle, which, as shown above, respects the sun in all its movements, also recedes westwards—in the northern hemisphere—from the meridian of the place by the space of one day's westward swing in a solar year. From this point of view, one can clearly discern, that our theory admit, that the magnetic equator of a planet lies direct in the plane of the equator of the sun, hence, in the case of our earth, it inclines to the ecliptic, according to Dr. Herschel, by the angle of 7° 20′. But the axis of the ecliptic inclines to that of the earth's equator by the angle of 23° 27′ nearly, from which take the angle 7° 20′, and there remains 16° 7′ for the inclination of the earth's equator to that of the sun, which is the very degree given by Dr. Mayer as the mean inclination of the magnetic equator to the terrestrial, as found on actual observations.

Now, it is evident that that magnetic meridian which passes through the node,

or point of intersection of these two equators, is at right angles with the magnetic equator, and consequently inclines to the true meridian at that point by the same angle of 16° 7'. When the needle in its secular swing comes to this meridian—which I shall term the *prime*—the rate per year of declination should be of the greatest value, and its tropics, east and west, should decline from it by the same angle of 16° nearly.

Next I shall inquire, as to whether this accords with the observations already made by scientists. The following table gives the declination of the compass needle at London, with the mean rate of its motion as referred to periods of observation between 1580 to 1865, comprising a part of an easterly half, the whole of the westerly, and a part of the next westerly half vibration. (Sir Wm. S. Harris' Rudiments of Magnetism. Dr. Woad's Ed. page 258; also Dr. Lloyd of Dublin).

EASTERLY DECLINATION.

Years of observation.....	1580	1622	1660
Declination.....	11°5′	6°0′	0°0′
Rate per Year of Declinat.	0°7′	0°8′	0°10

WESTERLY DECLINATION.

Years.	1692	1723	1730	1765	1818	1852	1865
Decl..	6°0′	8°36′	13°0′	20°0′	24°36′	22°30′	20°44′
Rate p. Y.	11′	11.7′	11.5′	0.9′	0.0′	0.5′	0.7′

Here we see that the rate per year of the variation was greatest about 1723, the time the declination at London was 8° 36′, that the tropic was reached in 1818 when the rate per year was zero, and the declination from London 24° 36′ or about 16° from the point where the rate per year was the greatest, or the node of the two equators.

Now, this prime meridian, or that which lies in the plane of the sun's axis, and intersects the two equators at their nodes, must become an important line in terrestrial magnetism, for when the horizontal magnet, on its secular swing, passes over it, it is then at its greatest amplitude, or most distant point from its tropics, its rate per year the swiftest, and the daily vibration of the greatest value; and the nearer a station is to this line on the same magnetic latitude, the greatest in proportion is the visible range of its daily vibration.

And even this is not all. When the dipping needle, in its secular vibration, comes to this line, it is always in one of its tropics. This is, as I shall soon prove, the very line of its apsides.

I have now arrived at my evidence that the magnetic equator of the earth lies in the plane of the equator of the sun, and since the magnetic pole revolves about that of the earth, it is plain, that the magnetic meridian cannot, in all places, and at all times cut the magnetic equator at right angles; it can only do so at that place called the nodes of the two equators.

Sir Wm. Snow Harris, in the volume just alluded to, observes that the oscillation of the needle across the true meridian is variable, that the limit of its angular variation at London is 24° 36′. It seems that he also understood, that the limit is not of that amount at all places, that it is only so at London, and those places under the same meridian. In fact, this angular variation at any station depends on the distance of its meridian from the prime meridian—the difference of its declination at London from the prime meridian is 8° 36′, which added to 16° gives 24° 36′, the observed angular variation of the needle at London, when it arrives at its westerly station where the variation rate per year is zero.

I further discovered, that the extent of the mean yearly vibration at any station is equal to the daily vibration at the time the needle comes to the prime meridian. The rate of the vibration at any station, evidently increases or decreases with the rate per year at which the needle moves in that declination, which is as the square root of the declination itself; both the rate per year, and the extent of the swing is evidently greater in the plane of the prime meridian, even as the magnetic intensity is greater in the plane of the solar axis.

From what has been said, it is evident that the magnetic axis only advances in its orbit during the time the needle vibrates westward; for though the earth continues to move regularly in its orbit, yet, while the needle moves to the east the magnetic axis does not advance on the earth's surface, for it only advances westwards, as before shown, and as the needle, which is always coincident with the axis of the sun, only moves westward for about half of the time, the magnetic

axis, in the mean, only advances westward about 30′ per day, as the earth advances nearly a degree a day in the zodiac. So, all other causes eliminated, the whole daily advance of the needle would only amount to that arc. But there are other phenomena that should be taken into consideration. The declination of the needle, as said before, changes with the sun's declination, and also with the motion of the earth in its orbit. Dr. Bache in his "Magnetic Discussions," page 10, has this remarkable expression: "The annular vibration depends on the earth's position in its orbit. The diurnal variation being subject to an inequality depending on the sun's declination. The diurnal range is greater when the sun has north declination, and smaller when south declination; the phenomenon passing from one state to the other, about the time of the equinoxes." Also, the diurnal range apparently increases as the needle in its secular variation approaches the prime meridian. Mr. Graham, the discoverer of the diurnal variation, who, happily made this discovery in 1723, about the time when the needle was crossing this line, as seen in the table above, found the daily variation to range 30′, the amount we found above as the mean range in the northern hemisphere. Dr. Bache adds, page 12: "At, (and before and after) the principal maximum (of the annular variation) between six and seven in the morning, the annular vibration causes the north end of the needle to be deflected to the east in summer, and to the west in winter; at one p.m. the deflection is to the east in winter and to the west in summer. The range of the diurnal motion is thus increased in summer, and diminished in winter; the magnet being deflected in summer more to the east in the morning hours, and more to the west in the afternoon hours, or having greater elongation than it would have if the sun moved in the equator. In winter the converse is the case." He also says, page 13, in reference to the annular variation, that Gen. Sabine expresses himself as follows: "Thus, in each hemisphere, the annual deflections—those that change with the declination of the sun—concur with those of the mean annular variation for half the year, and consequently augment them, and oppose, and diminish them in the other half. At the magnetic equator, there is no mean diurnal variation; but in each half year the alternate phases of the sun's annual inequality constitute a diurnal variation, of which the range in each day is about 3′ or 4′, taking place every day in the year except about the equinoxes; the march of the diurnal variation being from the east in the forenoon to the west in the afternoon, when the sun has north declination, and the reverse when south declination." According to the same authority (Gen. Sabine), the annular variation is the same in both hemispheres, the north end of the magnet being deflected to the east in the forenoon, the sun having north declination, while in the diurnal variation, the north end of the magnet, at that time of the day, is deflected to the east in the northern hemisphere. In other words, in regard to direction, the law of the annular variation is the same, and that of the diurnal the opposite, in passing from the northern to the southern hemispheres.

Now, since I showed that the diurnal variation is of the same extent as the annular steps of the secular variation, we only gain half a day's motion of the sun in a whole year; for as the direction of the needle's motion in the night is to us in opposite direction to what it is in the day, so the secular motion in the southern hemisphere is contrary to that in the northern hemisphere, so as to cause the yearly variation to help the diurnal, and so augment the secular in the northern to the amount of nearly 4′, as showed before, which is the range of the yearly variation about the magnetic equator; so the secular swing of the needle in the northern hemisphere becomes 34′ per year nearly. Now, 180°—the whole swing from tropic to tropic—divided by 34=318 years, the secular period of a whole vibration in the northern hemisphere, which is the very period given by Dr. A. M. Mayer in that celebrated lecture, "The Earth a Great Magnet," alluded to before. As to the reason why the secular swing of the needle appears to follow the law of a pendulum swinging about the center of gravity of the earth, is, that while the needle describes those parts of its orbit about the eastern and western tropics, its motion is nearly

in the direction of the line of our vision. As the needle advances in its orbit, the course of its swing makes a greater angle with that line, so as to appear to move swifter and swifter, until it arrives at the meridian of the station; where its sweep is at right angles to our vision line, and its velocity appears the greatest of all.

OF THE SECULAR MOVEMENT OF THE MAGNETIC NODES.

This motion may be termed "the most grand magnetic vibration." Since the magnetic needle in all of its movements respects the apparent motions of the sun, I thought it worthy of remark, that, from the phenomenon termed "the precession of the equinoxes," the nodes of the sun, or points where his path in the heavens cut the equinoctial, recede westward through the constellations of the zodiac, at the rate of about 50 sec. a year, which in connection with the eastward movement of the line of the apsides—12 sec. a year—performs a grand revolution in about 21,000 years; as the axis of the sun is thus carried westward around the earth, the magnetic nodes, or points where the sun's equator cuts the terrestrial, should also move at the same rate and in the same direction on the terrestrial equator, and so describe the same grand revolution from east to west in that vast period. And, not more strange than true, philosophers, long ago, observed this to be actually the case, though they could not account for it.

Sir Wm. Snow Harris, in the volume before alluded to, page 266, has the following remarkable expression: "By a careful analysis of the observations recorded at long intervals of time, the nodes, or points of intersection of the magnetic and terrestrial equators, have a slow westerly movement."

OF THE SECULAR VARIATION IN THE INCLINATION, OR DIP OF THE MAGNETIC NEEDLE.

From what has been explained with regard to the declination of the magnetic needle, it is evident that when such a needle is set to move freely, it always rests with its axis in the plane of the axis of the sun; which, as before demonstrated, revolves around the axis of the earth, in an orbit that declines from it by an angle of about 16°.

Now, if the earth were to revolve in the plane of the sun's equator, or that of any of its parallels, the dip of the needle would be always the same, in the same terrestrial latitude. But since the earth's orbit inclines to the sun's equator, and so the earth appears sometimes below, and sometimes above that plane, the magnetic pole of the earth, which is in juxtaposition to the pole of the sun, must appear to move alternately up and down on our meridians, according to what part of the orbit the sun appears to describe. And it is worthy of remark, that this phenomenon had long ago been observed by scientists to really exist, and termed "the secular variation of the dip of the needle." Though this phenomenon had been observed, the rate of its motion from time to time being watched, and its effect on the magnetic force and the movements of the isoclinal lines of the earth accurately determined by scientists, yet the extent of its vibration, the length of its period and the place of its tropics, had not been discovered by them.

Gen. Sabine observes, that it had been expected by many that the secular period of the dip's variation, which was then decreasing, would synchronize with that of the declination, and that the dipping needle would also come to its tropic in 1818; and that the dip would commence to augment from that period. But the philosophers had been disappointed in their expectation; the needle is still descending—the dip is still decreasing in the British Isles.

Now, the true amount of the variation of the needle from its mean at any station, is the same as the inclination of the axis of the ecliptic to that of the sun, which had been given before as 7° 20′. And since the needle always rests with its length in the plane of the solar axis, one might infer that its period is the same as that of the secular variation of the declination needle.

There is, to appearance, a vast disagreement between the periods of these two phenomena, but, by my theory, they should correspond; and, indeed, if we scrutinize their movement, there is the utmost correspondence—they exactly synchronize. The mistake remained, in

taking the meridian of London, for the goal to be sought for by the needle, instead of the prime meridian, or axis that passes through the intersection of the two equators.

The last period of the maximum of the inclination, or when the dipping needle came to its upper station, occurred in 1723, when the dip was 74° 42′ at London; this I call the upper transit of the needle over the prime meridian, where the dip is the greatest, from where the needle commences to fall, and the inclination diminishes in value for the space of 7° 20′. Now, if we consult the table given elsewhere, we will find that this year, (1723), was the very year the declination needle came to a coincidence with the prime meridian, where its declination to the true meridian was 16° 7′, and where the rate per year of its secular movement was the greatest of all.

By 1840, according to the observations made at Kew, the dip was 69° 12′, the difference in 116.7 years being 5° 28′ nearly, equivalent to an uniform diminution of 2′ 8 sec. annually, and Gen. Sabine observes that the rate of the diminution of the dip in London had not materially changed for the last 150 years.

The grand vibration of the declination needle, according to Dr. Mayer, is made in 318 years, half of which is 159 years, this multiply by 2.8=445′, or 7° 25′, the arc through which the needle falls, which is nearly equal to the given inclination of the ecliptic to the solar equator, 7° 20′. And I think the former is the most true measure of the latter, for it is evident, even if the latter was formerly correct, that as the inclination of the ecliptic to the earth's equator diminishes, its inclination to the sun's equator must increase by the same amount. Thus we see, that the secular period of the dipping needle is also the period of the declination needle; they were together on the prime axis in 1723, and will again meet on the same line in 1882, for 1723+159=1882, when the dip will begin to increase again.

I may here remark that to the east of the prime meridian, both the declination and the inclination of the needle increase in value till the needle arrives at its upper transit, whence, in describing the western hemisphere, they both decrease again.

One thing I have taken for granted in the above discussion—that the dip of the magnetic needle is double that of its magnetic latitude at any station—and as some modern scientists dispute the truth of this principle, and the propriety of its application to terrestrial magnetism, I shall make a few remarks thereon.

A few years ago, I independently discovered that the angular dip of the magnetic needle is double that of the magnetic latitude at the same station; but have since found that Mr. Kroft, of St. Petersburg, had long before deduced this law from his observation, and that Mr. Barlow, of England, subsequently arrived at a similar deduction by experimenting on a magnetic sphere of soft iron; that Biot endorsed it, and has given a formula for the inclination. I am pleased to yield the honor of the discovery to these wise men. But the explanation of the cause of this phenomenon I have not as yet met with.

It is represented in books, that at the magnetic pole the dip of the needle is 90,° and so it is to the horizon at that point; but not so in comparison to the horizontal needle at the magnetic equator. For, the earth being a globe, the position of the needle at the pole is "parallel" to that on the equator, its north pole points in the opposite direction, or it declines from the latter position by the arc of 180,° or twice 90° the greatest latitude.

It is a well known principle in optics, that, when a light is reflected from a rotating mirror, that the angle of reflection of a ray is double that of the rotating mirror, that is, if the mirror be made to rotate through 45° the reflected beam would pass through 90°.

If we now suppose the mirror to be a globe like our earth, it is evident that moving the beam around the globe from the equator to the pole would produce the same effect as causing the plane mirror to rotate. The same law is evidently observed by the dipping needle, in swinging its tail around the heavens, as it is carried in a free position from the magnetic equator to its poles.

The Secretary of State for India desires that the municipality of Bombay would urge the Government to carry out a system of drainage, as that would remove one source of ill-health and disease.

THE PROGRAMME OF THE STUDIES OF THE ARCHITECT AND OF THE CIVIL ENGINEER.

From "The Builder."

THE programme of the International Congress on Civil Engineering, lately reproduced in our columns, is not one that we can regard with entire satisfaction. As to its merit—as a compendious catalogue of the exhibits or contributions of any kind brought before the Congress, we have nothing to say. But we are entitled to expect that a document of this nature should form a sort of skeleton outline of the science of engineering. As such, especially when drawn up with the lucidity of phrase and systematic order which for the most part characterize French scientific works, such a paper might form a contribution of no little value to the science of higher education. As it is, however, the gaps and blanks are almost as conspicuous as the features illustrated. Thus, there is a head, "*Télégraphes pneumatiques*," but not a word as to the electric telegraph, or those wonderful methods now in process of daily improvement, by means of which the electric fluid is employed for the purpose of giving sonorous signals at a distance; or, in the words of Mr. Spottiswoode, electricity is converted into sound. Again, there is a heading "*Inondations: Moyens à leur opposer*," but not a word as to the first essential for carrying out any of these methods, the hydraulic survey of the district liable to the floods. Indeed, the whole question of survey, the very ground-work and basis of civil engineering, is omitted from the French programme.

We hold that a positive injury is inflicted on scientific education by the setting forth of partial details as if they constituted the whole of any branch of study. The tendency of the age is to run into detail. The division of labor is a means of acquiring intellectual, as well as physical wealth. But the danger of losing sight of the whole in elaborate detail of the parts is great and urgent. Unless the general form of a science or art be kept clearly before the attention of its students, they not only sink into mere specialists, but work in their special branches of study with less advantage than would be the case were their ideas enlarged, so as to appreciate the relation of their particular work to the general advance of the study of which it is an integral part.

We have been very much struck, within the past few weeks, with examples of the mode in which this specialisation of attention appears to have cramped and injured the *coup d'œil* of the architect. It is unnecessary to indicate localities, further than to say that we speak of a part of the country where pure air, noble prospects, good roads, and comparative sparseness of population are such as to prevent unusual inducements for the erection of private residences of a high class. Beautiful specimens of old English architecture stud the country, from the cottage and the farm to the baronial or knightly mansion. Men are found to understand these advantages, and to avail themselves of their existence. Money, it is certain, is forthcoming with an unstinted hand. A sort of paradise is open to the architect.

Yet here we find houses rising at costs varying from £1,500 to £15,000, or upwards, the inspection of which, as their plans gradually define themselves in brick, and stone, and mortar, serves to announce the absence of the architect—using the term in its highest sense. It is not that we have to complain of scamping, or of slovenly work. Quite the contrary. The details are often admirable. But the faults that we lament are the want of grasp, of breadth of plan, and of adapting the methods of the builder to the special circumstances of site. Here is a house that we might take as a model in many respects, with the stable-yard crammed—quite unnecessarily—so close to the main entrance as to shut off the garden view, and promise anything but salubrity to the reception-rooms, if the stud be more than a cypher. There we see three or four houses, each, may be, of some pretension to comfort and elegance, stuck so heedlessly in one another's light as to form an ill-adjusted

block, where there might have been a picturesque and self-contained group of residences. In another place we see a road so diverted as to cram one house into an ill-shaped triangular garden, commanded by two roads, while the attempt to obliterate the old road by the simple process of planting, without any reference to the rules of landscape or other gardening, has brought a bit of irredeemable Cockneydom into what was a little while since an elegant and picturesque country road. In another place, where at least from £15,000 to £20,000 must be in course of expenditure, where the site commands a magnificent view, and where the preparations for a terraced garden denote a great freedom from any narrow ideas as to cost, we find, rising in the air, instead of a noble mansion, a heterogeneous collection of rooms. A Gothic archway, that might serve for a church, opens into a little insignificant low vestibule, which entirely destroys the *raison d'être* of the gateway. Where a noble oriel window ought to command a broad and diversified view, a chimney is placed, with a small square glazed aperture, called by courtesy a window, on each side. By the doorway, a shapeless window, which looks like that of a buttery, is intended, by some strange caprice, to light a studio or drawing-room. All the details are admirable. No doubt some good examples may be cited for every mullion, every moulding, perhaps every room. But whole there is none—only a jumble of parts—and of parts that are petty and inappropriate, when the situation demands the simple and the grand.

Now we cannot doubt that an architect who, at the same time has so much and so little of what is required for excellence in his work as the author of this design, must be a sufferer from a want of that comprehensive, systematic, subordinated programme for his work, the want of which we lament in the Paris programme. Given a site of unusual beauty, and far-reaching view, the first duty of the architect should be so to arrange the chief rooms, and especially the windows, of the house as to take this view as much as possible within—to make it an unrivalled furniture of the reception apartments. Secondly, we might suggest, the idea of making the edifice a consistent and graceful pile of buildings, as forming part of the view from neighboring heights, should not have been forgotten. But to make use of such an opportunity for the sole purpose of reproducing Elizabethan mullions, thirteenth-century arch and mouldings, and quaint little windows out of which no one can look, is,—in our view of the case,—not only to waste money, but to sacrifice reputation.

With this view we will attempt to sketch out something of a rough programme of engineering study. Our work must be, necessarily, tentative and provisional. But those who may mend it, not by the criticism or the addition of mere details, but by giving a greater roundness, completeness, and system to the whole, will deserve well of their professional brethren and pupils.

The business of the engineer, then (to return to the Paris programme) contains three main divisions or provinces. These are (1) survey; (2) physical engineering; and (3) mechanical engineering. The head of special or unclassed studies may be added, provisionally, to include those pursuits which are in the course of rapid development, or which have not as yet been sufficiently advanced to be relegated to their appointed stations in the completed system of scientific order.

Survey is the basis of the whole science of engineering. It is either general or special. It ranges from geodesic operations of the first magnitude to the careful exclusion of a bit of sappy timber from a bridge or a door. The antiquity of the work of the surveyor has very recently been illustrated in an unexpected manner. An Assyrian tablet, in baked clay, has just been translated for our pages. It is a deed of sale of a plot of ground, and a plan of the ground in question is attached. This most ancient land survey is more than 2,000 years old. Had the plans of Rome, which were engraved on marble, been copied in terra-cotta, we might at this moment have a more accurate knowledge of the ancient topography of the Eternal City than we have of London in the time of the Conqueror. But it was not till the end of the last century that a trigonometrical survey was generally allowed to be the only accurate basis for mapping a country. General Roy began the trig-

onometrical survey of Great Britain by measuring his famous base on Hounslow Heath in 1784. In 1802, Major Lambton commenced the mathematical and geographical survey of India by measuring a base-line near Madras. Sir George Everest extended Lambton's "great arc series" across the plains of the Ganges, to the foot of the Himalayas; and when the vast peninsula had been covered with a gridiron of triangles, and a second base was measured in the valley of the Debra Dur, the difference between the computed and the measured length was only 7 inches. The height of the loftiest of the Himalayan peaks, named, in fit tribute to the great surveyor, Mount Everest, was determined by measurements of angles by the great theodolite as 29,002 feet above the sea.

Survey, then, forms the first part of the programme of the study of the engineer. It includes geodesic survey proper, or triangulation, with astronomical determinations of salient points; geographical and topographical delineation; orography, or the contours of the country; geological survey; hydrological survey; and hydrography, or preparation of charts of coasts and estuaries, including soundings and determination of tides and currents. Land survey is an important detail, subordinate to topographical delineation. The shading of hills and delineation of water-sheds, with the preparation of physical maps, ranks under the head of orography. The survey of buildings, and of quarries, mines, forests, and other sources of materials for the engineer and the builder, carries the duties of the surveyor to their limit of detail. We have not spoken of the pioneer surveyor, whose duty, though important, is only provisional.

Each branch of physical engineering is properly based on a branch of survey. The first call upon the engineer is for the establishment of communications. For this purpose, when the first stage of rough work is passed, the orographical and topographical surveys furnish the data. Communications at present are divided into national and international, or exterior and interior; divisions which partly, though not wholly, correspond with that of communication by land or by water. For the former, the engineer has to study the formation of roads, pavements, tramways, and railways; for the latter, he has to provide ports and harbors, to cut canals, and to systematize rivers.

The provision of internal waterways is closely connected with other branches of hydraulic engineering, based on hydraulic survey. Among them are drainage and irrigation—a study which requires for its completion the survey and regulation of forests and plantations. In the second place ranks the provision for the water-supply of urban districts, and, generally, of the population of the country. Inseparable from the water-supply question is that of sewerage, including the disinfection of its effluent water. Agricultural engineering must be considered in detail under a separate head, but is deeply affected by the system adopted for irrigation and drainage. The details of earthwork, masonry, timber, ironwork, and other element of construction, may be grouped together by the writer or lecturer, but will be studied practically by the pupil as they are carried out on the different public works of which the main characters are above indicated.

Mining, quarrying, coal-mining, well-sinking and boring, form a separate branch of study. It is related, on the one hand, to forestry and woodcutting; and, on the other hand, to metallurgy, smelting, and the making of iron and steel. The civil here comes into immediate contact with the mechanical engineer, whose cradle and school are found in the vast establishments which add forges to furnaces, and not only cast, roll, hammer, and forge, but also, turn, bore, and plane vast and complex objects of metal.

As the physical engineer gives his hand to the mechanical, so does the latter need much of the knowledge of the chemist. The study of heat has been usually regarded as a part of physics; that is to say, of that *remanet* of natural science which has not yet been portioned out under the name of a special study. But while, on the one hand, the study of heat, as far as its production and its metallurgic effect are concerned, is a part of industrial chemistry, the determination of the relation of heat to motion, which is one of the grandest strides of recent science, renders the

study of caloric a distinct part of mechanics. The English thermal unit—called after the name of its discoverer, Joule's equivalent—determines the equality of the energy required either to raise 772 lbs. for a foot, or to raise the temperature of 1 lb. of water by one degree of Fahrenheit's scale. This elevation of temperature in a pound of water can be produced by the consumption of half a grain of carbon. If water descends freely through a distance of 772 feet, it acquires from gravity a velocity of 223 feet per second; and if suddenly brought to rest when moving at this velocity, would be violently agitated, and raised one degree Fahrenheit in its temperature. So intimately connected are chemical, thermic, and mechanical phenomena. The study of mechanical engineering may be described as regarding, in the first instance, the application of natural sources of motion. These are water, wind, and animal power, to which the ingenious labors of Capt. John Ericsson have enabled the engineer to add the radiant heat of the sun.

The readiness with which the force of gravity can be utilized by falling water was perhaps one of the first discoveries in mechanics. The origin of the water-wheel is lost in the remoteness of antiquity. Still more ancient, no doubt, were the simplest contrivances employed, and to this day in use in India, for raising water for the purpose of irrigation. The construction of water-wheels—over-shot, breast, or under-shot—of turbines, or of any other apparatus for utilizing the mechanical force of a fall or current of water, is falling into neglect in our densely-peopled country. Certainty in command of power is even more essential to the owner of a large mill or factory than economy; and steam is displacing water as a prime motor for that reason. This is not, however, the case in America, in Italy, or in some other localities, where the water-mill is still a very important care of the engineer. In Great Britain, the disuse of water as a prime mover is likely to be fully made up for by its constantly increasing use as a transmitter of motion. The accumulator principle is one likely to exercise very wide development. Hydraulic rams, presses, gun-carriages, and second motors of all kinds are daily in course of new application. And the pump, with all its numerous applications, may be studied under this branch of engineering.

The service of wind as a motor power is falling still more rapidly into disuse than that of water. Long lines of windmills may still be seen pumping night and day, whenever there is a breath of wind stirring, to drain our eastern lowlands and fens; but the windmill is becoming more and more rare as a feature of English landscape. That use of the wind which, half a century ago, was one of the proudest peculiarities of the Englishman, whose insular home made him so often a born sailor, has received a last fatal blow from the opening of the Suez Canal. On the China trade, until that great waterway was opened, the sailing clipper ships competed successfully with steamers; the former passage occupying from 90 to 100 days, as against 75 to 80 days for the latter. On this well-known sea-path the course of the winds could be very clearly anticipated. But ships now run on the Australian line which perform a voyage exceeding 12,000 nautical miles, at an average speed of 11 or 12 knots, and consume only 1,500 or 1,600 tons of coal to drive a weight of 6,000 to 7,000 tons from port to port. Very few sailing vessels of any size are now building; and it is only the yachtsman or the fisherman who is likely long to spread his sails to the wind.

The use of compressed air, as a communicator of motion, however, is advancing together with that of water. The ingenious effort made some thirty-six years ago to avoid the great cost of the self-traction of the locomotive by a pneumatic apparatus, failed, not from mechanical, but from physical causes. As soon as the air in the tube was rarified by the action of the air-pumps, the heat of the earth rushed in, and restored the tension. Thus, the South Devon engines were at work, not only in drawing trains, but in pumping heat out of the earth; and they became almost red-hot in consequence. The use of air as a secondary motor is in its infancy. In some cases, as in mining and tunneling, highly compressed air performs the double function of moving the perforators, and of ventilating and cooling the

GENERAL LIBRARY UNIVERSITY OF MICHIGAN

works by its escape. It is probable that the employment of compressed air will hereafter receive a great development.

As to the use of animal power, the great object of the engineer at the present day is to dispense with its employment. Among the earliest steps in civilization may be reckoned the attachment of the bullock to the plough; and, much later, that of the horse, not only to the plough, but to the wagon, the boat, and the coach. The entire period comprised in the history of the application of animal power has witnessed an increase in velocity of work or of transport, from about one mile and a third to sixteen miles per hour. The former is the pace of the bullock in the plough; the latter rate of progress, that of a horse, at the fastest trot, was attained by some of the fastest of the English coaches forty years ago. One great disadvantage of animal power is that its cost increases rapidly together with the speed attained. It is not the work done which is the limit of expense; but the wear and tear of the animal tissues. Each creature has its natural pace, or rate of movement; and the most rapidly moving are also the lightest animals, and those least adapted for performing mechanical work. With machinery the reverse is the case. Speed, in machines, is a great element of cheapness. A machine driven twice as fast as another may do twice as much work in the same time; and although the consumption of fuel is proportioned to the work done, much of the other expense will be proportioned to the time occupied in doing it, so that the financial saving becomes considerable. Indeed, if experiments described in the *American Journal of Science and Arts* may be relied on, certain kinds of friction, such as that of journals, decrease with an increase of speed in the revolution of the machinery; a speed of surface revolution of 1 foot per minute giving 15 as a co-efficient of friction, and a speed of 100 feet per minute giving a co-efficient of only five. This diminution of cost accompanying increase of speed is an element which tends to the entire displacement of animal by mechanical moving power. It substitutes the steam-engine, or the caloric-engine, for the bullock or the horse, as the slave of man. Little by little it will extinguish the laborer, or the uninstructed man who derives his pay from the sheer exercise of muscular strength. Not a year passes without the substitution of mechanical power for human labor in some new field. The revolution thus in progress is one of more moment than any that the world has yet witnessed. Very long was it stoutly resisted—and resisted by the very men whose position, it may be hoped, will be elevated by the removal of the burden of toil from their shoulders. This fierce opposition has of late slackened, if not ceased, in this country. It is now rather felt to be the case, very often, that necessary work is shirked, or grudgingly performed, than that the laborer insists on his monopoly of toil. We here touch on a question in which the functions of the engineer bring him into contact with the statistician, with the statesman, and with the philanthropist. But while in newly settled countries, and in sparsely peopled districts, human muscles, and the ready service of the bullock, the horse, the ass, and even the llama, may long retain their present importance as prime movers and sources of power, there seems every reason to anticipate that neither water, wind, nor animal power will be employed as prime movers, except under rare and exceptional cases, in the engineering of the future. To one great exception, however, we have by-and-by to allude.

We cannot do justice to the subject without returning to its discussion. But in closing for the present, we cannot omit to express lively satisfaction at the manner in which the appreciation of the importance of exhaustive and systematic programmes is evinced by the first speaker at the meeting of the British Association. The address of the president is one of which every Englishman may feel proud. Mr. Spottiswoode's tacit protest against a professedly positive, but really negative, attempt to draw a hard-and-fast line to what is to be known, will receive the support of every worker in science, as contrasted with the dreamers in philosophy. Professor Ingram, in his apology for political economy, has taken up our own position—"That the study of the economic phenomena of society ought to be systematically combined with that of the

other aspects of social existence; that the excessive tendency to abstraction and to unreal simplification should be checked; that the *a priori* deductive method should be changed for the historical; and that economic laws and deductions from them should be examined, and expressed in less absolute form.

Nor must we omit, in calling attention to the accordance between the views we have long maintained and those now authoritatively put forth at Dublin, to congratulate the president of the Mechanical Sciences Section, Mr. Easton, C.E., on his advocacy of our own proposal, several times urged, for the creation of an administrative department "charged with the duty of collecting and digesting for use all the facts and knowledge necessary for a due, comprehensive, and satisfactory dealing with every river-basin or water-shed area in the United Kingdom."

WATER ENGINES *VS.* AIR ENGINES.

By L. TRASENSTER, of the University of Liége.

Translated from "Revue Universelle des Mines" for VAN NOSTRAND'S MAGAZINE.

I.

If we take no account of the heating due to the compression of air, the ratio of the work restored to the work expended, is expressed by

$$E=\frac{1-\frac{1}{n}}{2.303 \log. n'}$$

n being the number of atmospheric pressures.

[*Note.*—The deduction of this formula is given by M. Trasenster in a former article, as follows:

Let p=atmospheric pressure per square meter=10333 kilos.

P=pressure of the compressed air

v & V=volumes corresponding to above pressures

$P=np$

$V=nv$

The theoretical work afforded by compressed air is

$$T_r=(P-p)\,v=Pv-pv$$

but as $Pv=pV$ and $V=nv$

we shall have

$$T_r=Pv-pv=p\,(V-v)=pV\,(1-\frac{1}{n})$$

Whatever the pressure therefore to which one cubic meter of air be compressed the work performed by its expansion will always be less than $p\times 1$ or 10333 kilogrammeters. To accomplish this amount of work $\frac{1}{n}$ must become equal to 0, whence n equal to infinity.

The work of compression is expressed by the formula

$$T_d=pV+\text{nep. log. } n$$

and the ratio of work restored to work expended is as above

$$E=\frac{1-\frac{1}{n}}{pV\times 2.303 \text{ log. } n}.]$$

If the heat be taken into account the useful results are still lower and the losses augment with the pressures.

If c represent the specific heat of air at constant pressure

& c' represent the specific heat of air at constant volume

$$\frac{c}{c'}=k=1.408$$

If p represent pressure and Q the corresponding volume, then by Mariotte's law, pQ is a constant. Also according to Poisson pQ_k is a constant

Furthermore the coefficient of dilatation of gases by heat being $\frac{1}{273}=\frac{1}{a}$ and absolute zero being $-273°$ we have the known relation

$$\frac{273+T}{273+t}=\left(\frac{P}{p}\right)^{\frac{k-1}{k}}=\left(\frac{Q}{q}\right)^{\frac{-1}{k}}$$

and representing

$$\frac{k-1}{k}=\frac{0.408}{1.408}=0.29 \text{ by } b$$

we may write the above

$$\frac{273+T}{273+t}=\left(\frac{P}{p}\right)^{0.29}=\left(\frac{P}{p}\right)^{b}=n^{b}$$

From this supposing the initial temperature of the air 10° C we deduce the following values for temperatures for the several pressures given

P=	2 atmo.,	T=	73°
P=	3 "	T=	116°
P=	4 "		150°
P=	7 "		236°
P=	10 "		276°
P=	25 "		451°

If compressed air be expanded a lowering of the temperature is the result, the extent of which may be calculated by the same formula, T representing the initial and t the final temperature. If T=10° and the expansion be the result of diminishing the pressure from 3 to 2, the value of t becomes −21.4°. If T be 25°, t=−8.1°.

If a volume of air, compressed by 7 atmospheres as at Mont Cenis, and St. Gothard, be expanded to atmospheric pressure, we find by the formula

$$\frac{273+10}{273+t}=(7)^{0.29}$$

$$t=-112°.$$

The work absorbed by the compression is, taking the temperatures in account

$$T_a=\frac{k}{k-1}pQ\frac{T-t}{a+t},$$

But we know that

$$\frac{a+T}{a+t}=\left(\frac{P}{p}\right)^{\frac{k-1}{k}}=n^{b},$$

From which we get

$$\frac{T-t}{a+t}=n^{b}-1$$

and as

$$\frac{k}{k-1}=\frac{1}{b}$$

we shall have by substitution

$$T_a=\frac{1}{b}pQ\left(n^{b}-1\right).$$

The value of the work restored is

$$T_r=pQ\left(1-\frac{1}{n}\right).$$

Consequently we get for the ratio of work restored to work expended

$$E=\frac{pQ\left(1-\frac{1}{n}\right)}{pQ\frac{n^{b}-1}{b}}=\frac{b\left(1-\frac{1}{n}\right)}{n^{b}-1}.$$

and substituting for b its value 0.29

$$E=\frac{0.29\left(1-\frac{1}{n}\right)}{n^{0.29}-1}$$

The useful effect decreases as the pressure increases, and the more rapidly if we allow the air to heat during compression.

The following table exhibits the difference of useful effects of 1st, the compressed air cooled and, 2d, the compressed air allowed to retain the heat due to compression :

Pressures.	Useful effect. Air cooled.	Useful effect. Heat retained.
2 atm.	0.72	0.65
3 "	0.61	0.52
4 "	0.54	0.44
5 "	0.50	0.39
7 "	0.44	0.31
25 "	0.30	0.18

These figures show that not only is the useful effect diminished as the pressure increases, but that the difference between the performances of these two conditions augments also.

A pressure of seven atmospheres was employed in tunneling the Alps, and the pressure of twenty-five atmospheres has been recommended by M. Mèkarski for tramway engines.

The effect of heating has been largely avoided by the use of water spray as employed by M. M. Colladon, Cornet, and others. Diagrams obtained under such conditions differ but little from those required by Mariotte's law.

It is necessary in order to reduce the loss of work to a minimum to employ the expansive force of the air without so great loss of heat; but the problem presents great difficulties.

M. Cornet who has given much attention to all the practical questions relative

to compressed air, has suggested the use of an injection of water at the temperature of the mines. It has also been proposed to heat the outside of the cylinder, a plan of slight efficiency. Finally it has been proposed to employ, in connection with the compressed air, water heated to a high pressure.

II.

EMPLOYMENT OF WATER AT HIGH PRESSURE.

Compressed air possesses exceptional advantages as a motor for machines working at high velocities in shafts and galleries of mines. Its use, however, involves an expensive equipment, and it is rare that more than a third of the power of the compressing engine is realized in practice. It is, therefore, not an economical method of transmitting force to the depths of mines and tunnels.

Water, by reason of its incompressibility, transmits force without other loss than such as arises from friction. In mines its weight suffices for an initial force, without aid of special devices; but its mass prevents the use of high velocities in water pressure or piston engines. It is necessary, therefore, that in conducting pipes it should move with lower velocities than air or steam.

Notwithstanding the difference in density and mobility of the two fluids, the loss of work due to friction in the pipes can be made as little or less than that from use of air in two ways:

1st. By increasing the diameter of the conducting pipes, and thus reducing the velocity.

2d. By compensating for the diminution of velocity or volume of the water by an increase of the effective pressure without modifying the section of the conduits.

We know that for a circular conduit whose length$=b$, radius$=r$ and delivering a volume Q per second, the velocity

$$V=\frac{Q}{\pi r^2}$$

The *head* which measures the resistances to this motion is calculated by the formula,

$$h=\frac{2c}{r}lV^2 \text{ or } 2\ cl\frac{Q^2}{\pi^2 r^5}$$

The coefficient c is determined by experiment; for gas it is 0.00031; for water it varies between 0.000356 and 0.000385 according to the velocity, 0.00037 may be considered a mean value.

The height being thus determined, the pressure due to this upon a unit of surface is found by multiplying by the weight of a unit of volume. In other words, to calculate the pressure to the square meter it is necessary to multiply the height which measures the friction by the weight of a cubic meter of the fluid.

The weight of a cubic meter of water is 1000 kilograms. A cubic meter of air at 0° and pressure of $0^m.76$ is $1^k.293$. But the temperature is generally above this and it moreover contains a quantity of watery vapor so that the weight of the meter, under ordinary circumstances, may be taken at $1^k.25$ corresponding to a temperature of 9.4°. This is $\frac{1}{800}$ of the weight of the same volume of water.

Under a pressure of n atmospheres a cubic meter of air will then weigh $1.25n$ kilograms.

The pressure per square meter for air is

$$h\times 1.25n=\frac{2c}{r}lV^2\times 1.25n=2cl\frac{Q^2}{\pi^2 r^5}\times 1.25n$$

and for water,

$$h'\times 1000=\frac{2c'}{r'}lV'^2\times 1000=2c'l\frac{Q^2}{\pi^2 r'^5}\times 1000.$$

Equating these values;

$$\frac{2clQ^2}{\pi^2 r^5}\times 1.25n=\frac{2c'lQ^2}{\pi^2 r'^5}\times 1000$$

or

$$\frac{c\times 1.25n}{r^5}=\frac{c'1000}{r'^5}$$

or

$$\frac{0.00031\times 1.25n}{r^5}=\frac{0.00037\times 1000}{r'^5}$$

From which we get

$$\frac{r'^5}{r^5}=\frac{1000}{1.25n}\times\frac{37}{31}=\frac{800}{n}\times 1.193.$$

If $n=4$ we find

$$r'^5=r^5\times 200\times 1.193$$

or

$$r'=r\sqrt[5]{238.6}=r\times 2.989$$

Whence we see that it will suffice to triple the radius of the conducting pipe, in order to insure a circulation of water through it by the same effort or moving force as that required for the same vol-

ume of air under a pressure of four atmospheres; a medium $\frac{1}{250}$ of the density of water.

But a better solution of the problem is obtained in another way.

The ratio of the work lost by friction in the pipe, to the work afforded by the water at a pressure of n' atmospheres is

$$\frac{Q'h'1000}{Q'\times 10333n}=\frac{h'1000}{10333n'}$$

The loss then is a fraction which decreases as n' increases.

In the case of compressed air the ratio of work of friction to effective work is

$$\frac{Qh\times 1.25n}{Q\times 10333(n-1)}=\frac{1.25\ nh}{10333(n-1)}$$

It diminishes with the fraction $\frac{n}{n-1}$ and not with $\frac{1}{n}$ as in the case of water.

But the chief advantage of employing water at high pressure is that we obtain the same effective work as with compressed air, with so much less volume and can, consequently, reduce the velocity in the supply tubes in like proportion.

The work of a volume Q′ of water under a pressure of n' atmospheres or $n'p=n'10333$ kil. is expressed by

$$Q'n'p.$$

If we deduct the work of friction in the pipe,

$$Q'n'p-2\times 0.372\frac{Q'^3}{\pi^2r^5}$$

To make this work equivalent to that of a volume, Q of air, urged through a tube of the same dimensions, and with equal resistances for the two fluids, we establish the following equations:

1st. Equalizing the energy on entering the pipe:

$$Q'n'p=Q(n-1)p$$

or

$$Q'n'=Q(n-1);$$

2d. Equalizing the loss from friction in the pipe:

For water this work is

$$Q'h'\times 1000=\frac{2\times 0.00037}{\pi^2r^5}lQ'^2\ 1000.$$

For air it is

$$Qh\times 1.25n=\frac{2\times 0.00031}{\pi^2r^5}lQ^2\ 1.25.\ n$$

Equating these

$$Qh\times 1.25n=Q'h'\times 1000$$

or

$$\frac{2\times 0.00031}{\pi^2r^5}lQ^2 1.25n=\frac{2\times 0.00037}{\pi^2r^5}\times Q'^2\times 1000$$

or

$$31Q^2 1.25n=37Q'^2 1000.$$

we then have

$$Q^2=\frac{37}{31}\frac{1000}{1.25n}\times Q'^2=1.193\times\frac{800}{n}Q'^2$$

and

$$Q=Q'\sqrt[2]{\frac{954.4}{n}}.$$

For $n=2$ $Q=Q'\times 7.816$
" $n=3$ $Q=Q'\times 6.828$
" $n=4$ $Q=Q'\times 6.20$
" $n=5$ $Q=Q'\times 5.758$
" $n=6$ $Q=Q'\times 5.419$
" $n=7$ $Q=Q'\times 5.148$

The equation $Q'n'=Q\ (n-1)$ gives

$$n'=\frac{Q}{Q'}(n-1).$$

and consequently

atm.

For $n=2$ $n'=1\times 7.816=3.91\ n$ or 7.82
" $n=3$ $n'=2\times 6.828=4.55\ n$ or 13.65
" $n=4$ $n'=3\times 6.20\ =4.65\ n$ or 18.60
" $n=5$ $n'=4\times 5.758=4.61\ n$ or 23.03
" $n=6$ $n'=5\times 5.419=4.51\ n$ or 27.09
" $n=7$ $n'=6\times 5.148=4.41\ n$ or 30.89

Thus with pipes of the same diameter, a volume Q of compressed air, and a volume Q′ of water will yield the same effective work if

$$\frac{Q}{Q'}=\sqrt[2]{\frac{954.4}{n}}$$

if also the pressures n' of the water and n of the air bear the ratio

$$\frac{n'}{n-1}=\sqrt[2]{\frac{954.4}{n}}$$

It appears also that to realize this condition that the water pressure should not exceed 4.65 times the pressure of the air.

Another point of interest relating to water pressure or compressed air motors working in mines, is the influence of the difference of level between the two extremities of the conducting pipe.

If we suppose a vertical tube of a height H; n and N being the respective air pressures at the two extremities, we shall have the following relation:

$$\text{Log}\,\frac{N}{n}=\frac{1.25\times H}{10333}$$

calling $1_k.25$ the weight of a cubic meter of air.

But the results of this formula differ so little from those obtained by considering the air incompressible, that we may, for all ordinary cases, calculate the increase of pressure, per unit of surface, at considerable depths by estimating the column of air by $1.25+n\times H$. This is expressed in atmospheres per square meter by dividing by 10333

or $$\frac{1.25\times n\times H}{10333}$$

This for $n=4$ and $H=100$ meters is

$$=0.0484$$

consequently $N=n+0.0484=4.0484$

the logarithmic formula above gives

$$N=4.049$$

a difference of only 0.0006 of an atmosphere for a difference of level of 100 meters.

For 1000 meters the formulas give respectively for values of N; 4.484 and 4.516; a difference of only 0.032 of an atmosphere.

We may then in applying the formula to mines treat the air as we do water, and consider the augmentation of pressure at the bottom as due to the weight of a column of fluid of the same density throughout.

So that for a column of vertical height H we have for pressure per square metre due to height,

for air $H\times 1.25\times n$

for water $H\times 1000$

This pressure is reduced, 1st, by the friction of the fluids; and, 2d, by the counteracting pressure of the atmosphere or rather of the increase of atmospheric column. This latter would be the same for both kinds of motor and would be equal very nearly to 1.25 H kilograms per square meter.

The resistance due to friction is for the air, represented by a column equal to

$$\frac{2c}{r}HV^2,$$

and by a pressure equal to

$$\frac{2c}{r}HV^2\times 1.25\ n$$

For the pressure lost would be equal to

$$\frac{2c'}{r}HV^2\times 1000,$$

the velocity V being the same in both cases.

Consequently the pressures, after making the deductions, would be

For air

$$H\times 1.25\ n\left(1-\frac{2c}{r}V^2\right)-H\times 1.25.$$

For water

$$H\times 1000\ \left(1-\frac{2c'}{r}V^2\right)-H\times 1.25.$$

If we make

$$n=4,\ V=1 \text{ and } r=0.10,$$

we shall have the effective pressure, for air,

$$5H(1-0.0062)-1.25\ H=H(5\times 0.9938 -1.25)=H\times 3.719$$

and for water,

$$H\times 1000(1-0.0074)-1.25H=H(992.6 -1.25)=H\times 991.35.$$

If $H=100$ we shall for pressure per square meter, due to difference of level; for the air 371.9 kil. which for $V=1$ and $r=0.10$ would represent a supplementary work of $371.9\times 0.0314=11.68$ kilogram-meters, or 0.156 horse-power.

With water the supplementary work for the same conditions would be:

$$99135\times 0.0314=3112.82\ ^{km}.$$

$=41.50$ horse-powers or 266 times as much as from the same volume of air at four atmospheres pressure.

For a pipe of $0^m.05$ radius and a velocity of one meter, the effective work of water at 100 meters becomes 10.3 horse-power; at 400 meters it becomes 41.2, and if for this depth the radius is made $0^m.10$ the effective work=166 horse-power.

It is true that in most cases the water used for such purpose in mines would require pumping out again; but this requires no unusual equipment. The drainage of mines by pumping engines is a constant factor of mine working. These engines are usually steam pumps yield-

ing an efficiency of 75 to 80 per cent. of the power of the engine.

Compressed air, on the other hand requires for the compression, a special apparatus, in which not more than a third of the work is rendered effective.

The greater pressures required for water motors would demand stronger and more costly tubes. But it may be added that in working the galleries of mines a descent of the water from the motor to the well of the drain pump would frequently afford a source of power.

A recapitulation of the foregoing is exhibited in the following formulas:

The effective work of air compressed without heating is

$$E=\frac{1-\frac{1}{n}}{2.3\log n},$$

and

$$E=\frac{0.29\left(1-\frac{1}{n}\right)}{n^{0.29}-1},$$

when we consider the air heated to the full extent due to the compression.

Water meets in the pipes greater resistances than air; but for the same volume transmitted the loss of work from this cause is the same for the two fluids if the radii of the conduits have the ratio:

$$\frac{r'}{r}=\sqrt[5]{\frac{1193}{1,25n}};$$

the weight of the cubic meter of air being 1.25 kil.

Both air and water in conduits of the same diameter yield the same effective work at the ends if the volumes Q and Q′ and the pressures n and n' bear the following proportions:

$$Q=Q'\sqrt[3]{\frac{954.4}{n}}$$

$$n'=\frac{Q}{Q'}(n-1)=(n-1)\sqrt[3]{\frac{954.4}{n}}.$$

Finally, in a descending column the increase of useful pressure per square meter due to the weight of the fluid is, for a height H and velocity V, for air

$$H\times 1.25n\left(1-\frac{0.00062}{r}V^2\right)-H.1.25$$

and for water

$$H\times 1000\left(1-\frac{0.00074}{r}V^2\right)-H.1.25.$$

We may conclude then that although compressed air possesses undoubted advantages as a motive power in mines, where machines run with a high velocity and a shock, as do the several drilling machines, for ordinary service the high pressure water engines are preferable on the score of efficiency and economy.

THE MOST ANCIENT LAND SURVEY IN THE WORLD.

From "The Building News."

HERODOTUS, the father of history, tells us that the science of geometry originated in Egypt, where the practice of land-surveying was first rendered necessary by the frequent obliteration of landmarks, through the periodical overflows of the river Nile. Plato ascribes the invention of geometry to Thoth. Iamblichus says that it was known in Egypt during the reign of the Gods; and Eustathius, in speaking of an age long before the Greeks were sufficiently advanced to study or practice the art, says that the Egyptians "recorded their march in *maps*, which were not only given to their own people, but to the Scythians also, to their great astonishment." The frequent changes of surface must have rendered the land-surveyors' a rather busy profession in ancient Egypt, and a considerable body of them were employed by Rameses III., whose office is thus described by Herodotus: "If the river carried away any portion of a man's lot, he appeared before the king and related what had happened, upon which the king sent persons to examine, and determine by measurement the exact extent of the loss;

If we suppose a vertical tube of a height H; n and N being the respective air pressures at the two extremities, we shall have the following relation:

$$\text{Log}\,\frac{N}{n}=\frac{1.25\times H}{10333}$$

calling $1_k.25$ the weight of a cubic meter of air.

But the results of this formula differ so little from those obtained by considering the air incompressible, that we may, for all ordinary cases, calculate the increase of pressure, per unit of surface, at considerable depths by estimating the column of air by $1.25+n\times H$. This is expressed in atmospheres per square meter by dividing by 10333

or $$\frac{1.25\times n\times H}{10333}$$

This for $n=4$ and $H=100$ meters is

$$=0.0484$$

consequently $N=n+0.0484=4.0484$

the logarithmic formula above gives

$$N=4.049$$

a difference of only 0.0006 of an atmosphere for a difference of level of 100 meters.

For 1000 meters the formulas give respectively for values of N; 4.484 and 4.516; a difference of only 0.032 of an atmosphere.

We may then in applying the formula to mines treat the air as we do water, and consider the augmentation of pressure at the bottom as due to the weight of a column of fluid of the same density throughout.

So that for a column of vertical height H we have for pressure per square metre due to height,

for air $H\times 1.25\times n$

for water $H\times 1000$

This pressure is reduced, 1st, by the friction of the fluids; and, 2d, by the counteracting pressure of the atmosphere or rather of the increase of atmospheric column. This latter would be the same for both kinds of motor and would be equal very nearly to 1.25 H kilograms per square meter.

The resistance due to friction is for the air, represented by a column equal to

$$\frac{2c}{r}HV^2,$$

and by a pressure equal to

$$\frac{2c}{r}HV^2\times 1.25\ n$$

For the pressure lost would be equal to

$$\frac{2c'}{r}HV^2\times 1000,$$

the velocity V being the same in both cases.

Consequently the pressures, after making the deductions, would be

For air

$$H\times 1.25\ n\left(1-\frac{2c}{r}V^2\right)-H\times 1.25.$$

For water

$$H\times 1000\ \left(1-\frac{2c'}{r}V^2\right)-H\times 1.25.$$

If we make

$$n=4,\ V=1 \text{ and } r=0.10,$$

we shall have the effective pressure, for air,

$$5H(1-0.0062)-1.25\ H=H(5\times 0.9938 -1.25)=H\times 3.719$$

and for water,

$$H\times 1000(1-0.0074)-1.25H=H(992.6 -1.25)=H\times 991.35.$$

If H=100 we shall for pressure per square meter, due to difference of level; for the air 371.9 kil. which for V=1 and $r=0.10$ would represent a supplementary work of $371.9\times 0.0314=11.68$ kilogram-meters, or 0.156 horse-power.

With water the supplementary work for the same conditions would be:

$$99135\times 0.0314=3112.82\ ^{km}.$$

=41.50 horse-powers or 266 times as much as from the same volume of air at four atmospheres pressure.

For a pipe of $0^m.05$ radius and a velocity of one meter, the effective work of water at 100 meters becomes 10.3 horse-power; at 400 meters it becomes 41.2, and if for this depth the radius is made $0^m.10$ the effective work=166 horse-power.

It is true that in most cases the water used for such purpose in mines would require pumping out again; but this requires no unusual equipment. The drainage of mines by pumping engines is a constant factor of mine working. These engines are usually steam pumps yield-

ing an efficiency of 75 to 80 per cent. of the power of the engine.

Compressed air, on the other hand requires for the compression, a special apparatus, in which not more than a third of the work is rendered effective.

The greater pressures required for water motors would demand stronger and more costly tubes. But it may be added that in working the galleries of mines a descent of the water from the motor to the well of the drain pump would frequently afford a source of power.

A recapitulation of the foregoing is exhibited in the following formulas:

The effective work of air compressed without heating is

$$E=\frac{1-\frac{1}{n}}{2.3\log n},$$

and

$$E=\frac{0.29\left(1-\frac{1}{n}\right)}{n^{0.29}-1},$$

when we consider the air heated to the full extent due to the compression.

Water meets in the pipes greater resistances than air; but for the same volume transmitted the loss of work from this cause is the same for the two fluids if the radii of the conduits have the ratio:

$$\frac{r'}{r}=\sqrt[5]{\frac{1193}{1{,}25n}};$$

the weight of the cubic meter of air being 1.25 kil.

Both air and water in conduits of the same diameter yield the same effective work at the ends if the volumes Q and Q′ and the pressures n and n' bear the following proportions:

$$Q=Q'\sqrt[5]{\frac{954.4}{n}}$$

$$n'=\frac{Q}{Q'}(n-1)=(n-1)\sqrt[5]{\frac{954.4}{n}}.$$

Finally, in a descending column the increase of useful pressure per square meter due to the weight of the fluid is, for a height H and velocity V,
for air

$$H\times 1.25n\left(1-\frac{0.00062}{r}V^2\right)-H.1.25$$

and for water

$$H\times 1000\left(1-\frac{0.00074}{r}V^2\right)-H.1.25.$$

We may conclude then that although compressed air possesses undoubted advantages as a motive power in mines, where machines run with a high velocity and a shock, as do the several drilling machines, for ordinary service the high pressure water engines are preferable on the score of efficiency and economy.

THE MOST ANCIENT LAND SURVEY IN THE WORLD.

From "The Building News."

HERODOTUS, the father of history, tells us that the science of geometry originated in Egypt, where the practice of land-surveying was first rendered necessary by the frequent obliteration of landmarks, through the periodical overflows of the river Nile. Plato ascribes the invention of geometry to Thoth. Iamblichus says that it was known in Egypt during the reign of the Gods; and Eustathius, in speaking of an age long before the Greeks were sufficiently advanced to study or practice the art, says that the Egyptians "recorded their march in *maps*, which were not only given to their own people, but to the Scythians also, to their great astonishment." The frequent changes of surface must have rendered the land-surveyors' a rather busy profession in ancient Egypt, and a considerable body of them were employed by Rameses III., whose office is thus described by Herodotus: "If the river carried away any portion of a man's lot, he appeared before the king and related what had happened, upon which the king sent persons to examine, and determine by measurement the exact extent of the loss;

and thenceforth only such a rent was demanded of him as was proportionate to the reduced size of his land. From this practice, I think, geometry first came to be known in Egypt, whence it passed into Greece." Whether these ancient land-surveyors' made plans of the land they measured we cannot say, because among the copious records of Egypt no agricultural plans, so far as we can at present remember, have yet been found. There are some plans remaining of royal tombs, with dimensions carefully figured in cubits, and also of the turquoise mines of Wadi-Magarah, fac-similes of which have been published by the German Egyptologist, Dr. Lepsius; and there are verbal records of the boundaries of particular lands, but none of the maps mentioned by Eustathius, or of those which possibly were drawn by the surveyors of Rameses or their successors.

Discoveries recently made, however, at the British Museum among the cuneiform inscriptions on the terra-cotta tablets of ancient Babylon render it questionable whether the Babylonians should not have at least equal credit with the Egyptians, for the discovery of the science of geometry, and of its application to land-surveying and the delineation of plans. The country between the Euphrates and the Tigris was very early inhabited by a land-owning population, and was subject to the same vicissitudes of periodical overflow by the rivers as Egypt; and like circumstances produced similar effects upon their progress in science and arts. Laws for the regulation of property in land may be traced as far back as the days of the Kassite kings, B.C. 1656, which are written in the very earliest Turanian, or Accadian, dialect of the country, and which have just been translated by Mr. St. Chad Boscawen. Several curious particulars are found in these most ancient tablets. For example, it appears most clearly that the women of Babylonia could hold real property, that land could be mortgaged, and that it could be pledged, together with other things which modern civilization does not allow. Thus one tablet says: "His house, his grove, his field, his slaves, male and female, for silver he has pledged." We learn also that the interest charged upon these transactions was often as much as 30, and sometimes even 70 per cent.

The actual definition of the boundaries of land was effected in Baylonia by boundary stones, on which were carved not merely a statement of the boundaries, but words which constituted the stone itself the actual deed of gift or sale. One of the most noticeable of these boundary stones in the British Museum is a large stone bearing an inscription of Merodach-baladan I., B.C. 1200, presented by the proprietors of the *Daily Telegraph*. It records a gift by the King of a plot of land to a person named *Merodachsum Izakir*, as a reward for political services. It gives no dimensions, but carefully describes all adjoining properties, and is attested by many witnesses. Another conical black stone, dated B.C. 1150, is extremely interesting, as giving the price paid for the purchase of the field—viz., 616 *mana* of silver; but inasmuch as this price was paid in kind, not in cash, we have an enumeration of the different articles, with their respective values, among which are: "One chariot, with its harness, for 100 silver; six riding horses, equal to 300 of silver; a cow in calf, some asses and mules, as well as numerous pieces of cloth." This stone also gives us the name of the ancient land-surveyor, who not only defined the boundaries, but also assessed the value of all these chariots, cows and calves, and asses and mules. Let the land-surveyors of the 19th century learn to reverence the name of this man, who, until Mr. Boscawen unearths some still older tablet, must remain the father of their art. His name was Sapiku, the son of *Merodach-baladhu*, and he is expressly called *Masakhu*, the field-measurer.

The number of documents (that is, terra-cotta tablets) which the Museum now possesses in relation to the commercial and land transactions of ancient Babylon and of Assyria is very great, a collection of more than 2,000 having been purchased at Baghdad in 1875. Mr. Boscawen published an account of some of these last year in a literary contemporary,* showing that they formed a tolerably complete record of the business transactions of a great Babylonian firm,

* The *Academy*.

who traded under the name of Egibi & Sons, as bankers and state land agents. Their records relate to every kind of transaction—land sales and leases, loans of money, mortgages, sales of slaves, and dealings in all kinds of property—and the documents show that they traded in this manner from the first year of Nebuchadnezzar, B.C. 605, till the last of Darius Hystaspes, B.C. 486, a period of about 120 years. There are many interesting facts as to the daily life of the ancient people to be gathered from them, but that which it is our present purpose only to notice is the tablet which contains, not simply a description, but an actual *plan* of the land referred to in the document, just as plans are now drawn on parchment in the margins of leases. This, we think we may safely say, is at present the oldest known land-survey in the world. It is drawn on a tablet in dark terra cotta, about 6 inches by 3½ inches, and represents a plot of land about 8½ acres in area. The inscription at the top informs us that it is the plan of "A field in the high road on the banks of the river or canal," Nahr Banituv. The name of the river, however, is obliterated, and its place has been supplied by Mr. Boscawen from information drawn from other tablets relating to adjoining property. The estate is divided into three pairs of parallelograms, to which are added two more similar-shaped plots, and an irregular trapezoidal piece. The dimensions are all given in cubits, or fractions of cubits, most carefully figured on the drawing. Taking the Babylonian cubit as 20.475 English inches, the greatest length of the estate would be, from north to south, 1646 cubits, or 936 yards 0 feet 5 inches English. The width on the northern border on the edge of the highway is 84 cubits=140 feet. The dimensions on the southern part being much defaced, it is difficult to ascertain the length of the base line. On the east side the curve is most carefully measured, its circumference being 120 cubits, or 200 feet. A small dimension has been marked in the interior of the arc, which evidently represented its radius, but it is unfortunately obliterated. The northern boundary is the highway, or, as it is called in another document, "the royal highway." (It is interesting to notice such a very ancient use of our present common phrase, "the king's highway.") The western side adjoins the lands of Ipriya and Buruga, the son of Taria, the son of the Chief Builder, and this latter person is the owner also of the land on the southern boundary. The eastern side and the upper portion adjoin the lands of Nabu-sar-ibni, and another portion adjoins the lands of Kasiya, the son of Dibzir, the son of Pitu-sar-babi. It would seem strange for a modern surveyor to mark upon his plan, not only the name of his client's neighbors, but those of their fathers and grandfathers, yet this practice has revealed to us the fact that the ancient Babylonian "Chief Builder," or architect, was a person of some consequence, who left lands behind him, and grandchildren to be proud of their descent from him; and not the serf, or servant, which he was mistakenly represented to be in one famous modern picture.

As an example of the system of mensuration, and curious method of computation of the area, which was according to the amount of corn seed required to sow it, we make the following extract from a tablet dated in the third year of Nabonidus, king of Babylon:

1. 949 cubits on the upper side towards the west a boundary is fixed.
2. By [the land of] Nabu-sum-utsir, the giver of the field.
3. 949 cubits on the lower side towards the east the boundary is fixed by the land of Nabu-sar-ibni, son of Marducu.
4. 40 cubits the upper headland, a boundary line is fixed by the king's highway on the bank of the canal of Banituv.
5. 40 cubits the lower headland, a boundary is fixed by the other portion of the field.
6. For this field, and this portion, five measures of corn seed. A field with the wells attached.
7. A valuation of 5 epha., 8 measures of corn seed.

This is the first measurement.

This represents the measurement and sowing area of the first portion of the land sold in the tablet. A second portion which joins on to the southern border, is also computed by a similar arrangement. A summary of the two results is given, and the price in silver, according to the market value of corn, is computed and entered as the price of the land. A guarantee of about one-tenth per cent. is required and given as security for the fulfilment of the clauses of the

deed. The names of seven witnesses who attest the deed, by affixing their *nail-marks*, and the scribes, who append their seals, testify to the legal character of the document.

Such was the legal procedure in the conveyance of land 2,500 years ago in ancient Babylonia. How little it differs from the legal acts and deeds which are daily transacted in our modern Babylon of London, and in this Great Britain which has just assumed new responsibilities in relation to the old country whence these antiquities have been exhumed!

APPARATUS FOR DETERMINING THE RESISTANCE OFFERED TO SHIPS BY EXPERIMENTS ON THEIR MODELS.

By A. LETTIERI.

From "Rivista marittima," Abstracts published by the Institution of Civil Engineers.

This is an apparatus for experimenting on the resistance offered to the models of ships. The inventor considers that the determination of the resistance encountered by a vessel moving at different velocities in still water is a most important question, which has been solved by Mr. Froude. The law which this gentleman has formulated, by which to deduce the resistances met by a vessel from those encountered by its model, Signor Lettieri considers to have been fully verified by the experiments made by Mr. Froude on the "Greyhound" and its model.

The further prosecution of similar experiments Signor Lettieri thinks useful, or even necessary, with the view of ascertaining, before the launch of a vessel, the curve of the resistance that it will encounter with different loads and displacements. Being unacquainted with the apparatus used by Mr. Froude, Signor Lettieri has invented one of his own, the description of which he illustrates with a drawing.

In experiments of this nature the elements to be determined are two: the uniform velocity, and the resistance encountered at that velocity. The first of these is obtained by the measurement of the space passed through in a unit of time. It is, therefore, desirable to have an apparatus which shall graphically denote this velocity by a curve, and refer it to a measure of the resistance.

To effect this, Signor Lettieri has designed a vertical cylinder (the drawing shows the length to be fourteen times the diameter, but neither scale nor dimensions are given), which revolves on a fixed axis. The upper part of this axis sustains a pulley, and a second pulley is fixed beneath the cylinder, with a small drum on its axis. A line attached to the drum passes over the upper pulley, and sustains a scale pan, to which is fixed a pencil, the point of which presses against the cylinder. The model is attached by a line to the lower pulley, so that the descent of the weight corresponds to the movement of the model through the water; while the weight itself is a measure of the resistance. Movement is given to the vertical cylinder by means of a pair of conically toothed wheels, one of which is attached to the cylinder itself. The motion of the latter being thus made uniform, and its velocity known, the curve traced on it by the pencil will indicate the relation between the movement of the model and that of the cylinder, and will form a regular spiral when both movements are uniform. The remainder of the Paper is occupied by an algebraical investigation of the curves thus to be obtained, and by the relation between the weight placed in the scale pan, and the resistance encountered by the model in its passage through the water.

Fifty sailors were placed in one of Mr. Berthon's twenty-eight feet collapsing boats at Portsmouth, for the purpose of testing it. The sea was very lumpy, but the boat, which is capable of carrying eighty men, behaved perfectly to the satisfaction of those under whose superintendence the trial was made.

MECHANICAL CONVERSION OF MOTION.

By GEORGE BRUCE HALSTED.

Contributed to Van Nostrand's Magazine.

CAUSE AND DESIGN OF THIS PAPER.

By mathematicians in the last four years has been created a branch of their science, which is so practical that it seems as if its results need only to be put before mechanicians in order to produce very important applications.

The fact that these results have been, and could have been, attained only by mathematicians, has tended, we fear, to frighten away practical men from a subject, of which a great part is capable of being so simply put as to furnish at once a new and beautiful weapon in the field of mechanical contrivance. This should be of especial interest in America, the land of practical applications; and so we have attempted to bring here into connection the new achievements with some of the old ones they seem suited to supersede, confidently leaving the rest to that sharp-sighted ingenuity for which our land is famous.

HISTORICAL INTRODUCTION.

No way is perhaps better fitted to pleasantly awaken interest than the prefixing of a slight historical sketch of a chapter of progress, which seems to furnish a very beautiful example of how the torch of science is passed from hand to hand, from land to land.

It does not need an expert to appreciate the theoretical interest and practical importance of being able to draw a straight line, or convert a straight thrust into circular motion, and *vice versa;* yet perhaps one not acquainted with the subject will feel somewhat incredulous, when told that this was never accurately accomplished before the year 1864, when a method of doing it exactly was discovered by M. Peaucellier, then an officer in the French army. This method we intend to present and explain; but meanwhile we will trace briefly its history and progress.

FIRST ISOLATED FACT.

He first announced it in general terms, in the form of a question in the "Nouvelles Annales de Mathematiques," 1864. He did not, however, seem fully to appreciate the importance of what he had done; nor did his discovery catch the attention of any one prepared to see its value, so it fell into oblivion for six years.

Yet there was at this very time a great mathematician, Dr. Tchebicheff, in Russia, working on this very question, and, in fact, trying to prove the *impossibility* of the exact conversion of circular into rectilinear motion.

Now, it would be interesting to investigate how it came about, that in 1870, only six years after its first discovery, this wonderful conversion was re-discovered just in the right place, that is, in Russia, by one of Tchebicheff's own students, named Lipkine.

His professor obtained for this fortunate youth a substantial reward from the Russian Government; and this has since stirred up that most conservative body, the Institute of France, to confer its great mechanical prize, the "Prix Montyon," on Peaucellier, who gave, in 1873, a detailed exposition of his discovery, in the same journal which had published his first intimation nine years before.

Meanwhile Lipkine had presented the theory and description of his apparatus to the Academy of St. Petersburg in 1871, and exhibited a model of it at the Vienna Exposition in 1873.

THROUGH RUSSIA TO ENGLAND.

Some months after, Dr. Tchebicheff happened to visit England, and there Prof. Sylvester asked him about the progress of his proof of the impossibility of the exact conversion of circular into rectilinear motion. Tchebicheff answered that, far from being impossible, it had actually been accomplished, first in France, and subsequently by a student in his own class. He then made a rough diagram of the instrument, which consists of seven links. Shortly after this interview, Dr. Garcia, the eminent musician, and inventor of the laryngoscope, happened to visit Prof. Sylvester,

and being shown the drawing, brought under his cloak next morning to the Professor a model, constructed with pieces of wood fastened together with nails as pivots, which, rough as it was, worked admirably, and drew forth the most lively expressions of admiration from some of the most distinguished members of the Philosophical Club of the Royal Society.

Soon after, Prof. Sylvester exhibited the same model in the hall of the Athenæum Club to his friend Sir Wm. Thomson, "who nursed it as if it had been his own child; and when a motion was made to relieve him of it, replied, 'No! I have not had nearly enough of it: it is the most beautiful thing I have ever seen in my life.'"

THE DEVELOPED THEORY.

Prof. Sylvester's appreciation carried itself over from admiration to accomplishment. He changed what seemed an isolated fact into a grand theory. He proved that every possible algebraical curve may be described by link-work. In a lecture before the Royal Institution he stated that we are able to bring about any mathematical relation that may be desired between the distances of two of the poles of a linkage from a third, and are thus potentially in possession of a universal calculating machine.

He exhibited and worked a cubic-root-extracting machine constructed on this principle, and claimed to have given the first really practical solution of the famous problem proposed by the ancients, of the duplication or multiplication of the cube.

Fired by this lecture, two young Englishmen, graduates of Cambridge, Mr. H. Hart and Mr. A. B. Kempe, took up the subject, and have been carrying it on with brilliant success.

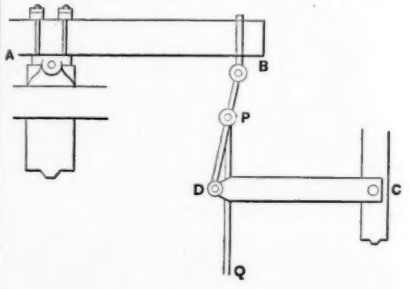
SOME RESULTS.

But now, perhaps, the reader begins to fear that our promise of simplicity was deceptive, and the subject must be too complex and difficult for a practical man.

This is very true in regard to its purely mathematical side;* but it is surprising how easily many of the results can be stated and explained to a person even entirely ignorant of mathematics, that dreaded science.

In addition to its theoretic interest, the direct importance of one of its applications is recognized when we consider, that in many machines and pieces of scientific apparatus, it is requisite that some point or points should move accurately in a straight line with as little friction as possible. If we are forced to use as guides planes ground smooth, the wear and tear produced by the friction of sliding surfaces, and the deformation produced by changes of temperature and varying strains, render it of real consequence to obtain, if possible, some more accurate and easy method which shall not involve these objectionable features.

As long ago as 1784, James Watt made an attempt, which was thus described by himself in the specification of a patent: "My second new improvement on the steam-engines consists in methods of directing the piston-rods, the pump-rods, and other parts of these engines, so as to move in perpendicular or other straight or right lines, without using the great chains and arches commonly fixed to the working beams of the engine for that purpose; and so as to enable the engine to act on the working beams or great levers, both by pushing and by drawing, or both, in the ascent or descent of their pistons. . . The principle on which I derive a perpendicular or right-lined motion from a circular or angular motion, consists in forming certain combinations of levers moving upon centers, wherein the deviations from straight lines of the moving end of some of these levers are compensated by similar deviations, but in opposite directions, of one end of other levers."



* For the literature of the subject, see the complete list given in my article "Historical Sketch of Exact Rectilinear Motion," *Van Nostrand's Mag.*, Jan., 1878.

AB is the working beam of the engine; PQ the piston-rod or pump-rod, attached at P to the rod BD, which connects AB and another bar, CD, movable about a center at C.

"When the working beam is put in motion, the point B describes an arc on the center A, and the point D describes an arc on the center C; and the convexities of these arcs, lying in opposite directions, compensate for each other's variation from a straight line; so that the point P, at the top of the piston-rod or pump-rod which lies between these convexities, ascends and descends in a perpendicular or straight line."

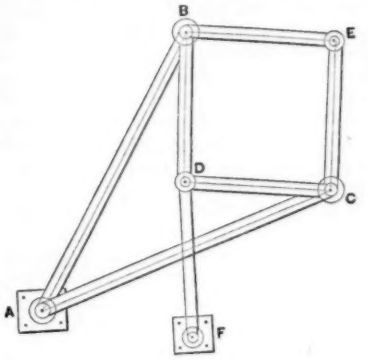
This would be most admirable if it were only true. In reality, the path of P lies on a figure 8, no part of which is straight; and it has been demonstrated that no combination of less than five links can enable us to get an accurate straight line, however short; while here, as we see, there are only three links, namely, AB, BD, DC.

The imperfection of Watt's movement led to other three-bar attempts and closer approximations; but with three bars it can never be solved. Still, if the swing of the beam of an engine be kept comparatively very small, the error will not be great; and so this Watt's Parallel Motion can be used, and we think still is used in the majority of English beam-engines, instead of the guides more usually employed in this country. That the guides can, however, thus continue successfully to compete with it, seems to us to depend upon the fact that it is necessarily inaccurate; and we see no reason why both should not be superseded by an application of one of the perfect rectilinear motions we desire to present.

FIRST ACCURATE SOLUTION.

The first accurate solution, as we have seen, was that of M. Peaucellier, in which seven links are used.

It consists of a rhombus composed of four equal links movably jointed at BCDE, and two other links movably pivoted at the fixed point A and at two opposite extremities BC of the rhombus. Take now an extra link FD, and pivot it to a fixed point whose distance from the first fixed point A is equal to the length of the extra link, whose other

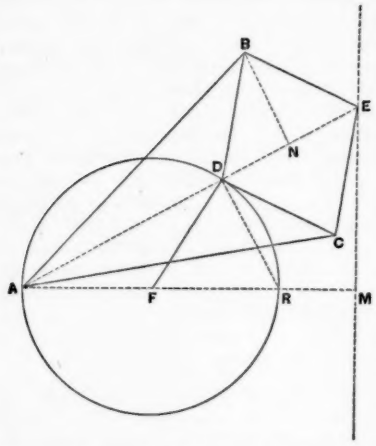


end is then pivoted to one of the free angles D of the rhombus. The opposite point E will now accurately describe a straight line, however the linkage be pushed or moved. The points B and C move in circles with radius AB, and the point D moves in a circle with radius FD, while E unvaryingly describes an absolutely accurate straight line perpendicular to a line joining A and F. So if we have our power in the form of the straight push of a piston, we have only to apply the end of the piston at E to have this straight push turned into circular motion at either of the other points we choose, and this too without the slightest tendency to side motion or wobbling, and consequently without any need of guides and their consequent friction and disadvantages. Again, if we have our power in the form of a circular motion and wish to transfer it to straight push or pull—for instance, to work a pump—we need only apply the circular motion at B, D, or C, to get perfect rectilinear motion at E.

PROOF OF ITS PERFECT ACCURACY.

All this may be rigidly proved by a little plane geometry as follows:

The angle ADR being always the angle in a semicircle, is always a right angle, and therefore the triangles ADR and AME having the angle at A common and the angles ADR AME equal, both being right angles, have consequently their third angles ARD AEM equal, and the triangles are similar. Therefore AD : AR :: AM : AE. Therefore AD . AE=AR . AM, moreover D may be on the circle.



But AR and AM once taken are constant, and their product AR.AM is a constant; so in order to devise a linkage such that when one of its points D is moved around in a circle, another of its points shall always remain on the identical chosen line EM, and shall consequently accurately describe that line, we must be able to discover such a linkage that however it may be moved, the product of the variable distances AD and AE shall always be exactly equal to the constant known product AR.AM, while in addition the movable point D always remains on the variable straight line AE. Now see how beautifully our linkage answers these difficult requirements and gives us the long-desired solution. On DE, the part of the line ADE which is exterior to the circle, construct, using DE as diagonal, any equilateral rhombus, as for instance BDCE, of four links jointed together so as to move easily. Pivot to B and C the two equal links AB, AC. Now from the symmetry of this linkage, however it be moved on its joints, the points A, D, E always are in a straight line, and the radius FD keeps the point D always on the given circle. Drop the perpendicular BN, and we always have DN=NE.

Now $AB^2=AN^2+BN^2$

$BE^2=EN^2+BN^2$;

therefore subtracting,

$AB^2-BE^2=AN^2-EN^2=(AN+NE).$

$(AN-NE)=AE.AD,$

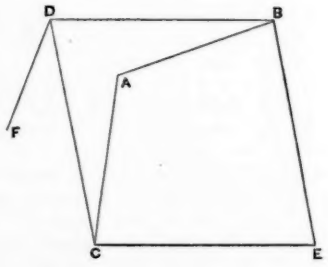
and since the bars AB and BE once made are of constant length, therefore the product AE.AD is constant, however much the distances AE and AD may vary individually as D is carried around the circle. Thus our desires are accomplished, and we have a machine for drawing straight lines, or turning circular into rectilinear motion, and *vice versa*.

A SUCCESSFUL APPLICATION.

Although this motion seems as yet almost entirely unknown to ordinary mechanicicans, yet it has been already applied in a beautiful manner to the air-engines which are employed to ventilate the Houses of Parliament in England.

The ease of working and absence of friction and noise are said to be very remarkable. Even the workmen there never tire of admiring their graceful and silent action. The engines were constructed and the Peaucellier apparatus adapted to them by Mr. Prim, the engineer to the Houses, of whom Prof. Sylvester tells the story that, conversing with him one day, just before the first engine was to be made, the Professor happened to mention that he supposed, of course, Mr. Prim knew that the point A need not be outside the rhombus but might be taken inside it, and the two equal bars thus made very compact. "Why! you don't mean to say so!" cried Mr. Prim. "Is it possible? Why then I can work it all from below, and won't have to knock a hole in the roof, as I thought I'd have to."

Prof. Sylvester gives this as an illustration of how an engineer of exceptionally good capacity will not see things which, to a mathematician appear perfectly obvious.



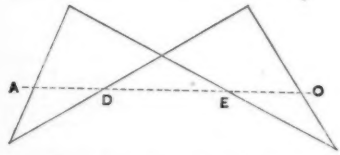
The form mentioned is given in the adjoining figure, where A and F are the fixed points and DF the extra link, the lettering of the two previous figures being retained. Omitting the extra link, this is called the negative Peaucellier cell, the one first given being called the positive cell.

ANOTHER APPLICATION.

Mr. Penrose, the eminent architect to St. Paul's Cathedral, has put up a house-pump worked by a negative Peaucellier cell, to the great wonderment of the plumber employed, who could hardly believe his senses when he saw the sling attached to the piston-rod moving in a true vertical line, instead of wobbling, as usual, from side to side. A sister pump of the ordinary construction stands beside it, but the former, although quite as compact as its neighbor, throws up a considerably larger head of water with the same sweep of the handle. Its elegance and the frictionless ease with which it can be worked (beauty, as usual, the stamp and seal of perfection) have made it the pet of the household.

RECIPROCATING PROPERTY OF CELL.

Now to return to our cell, we see that its peculiar power depends on the fact that, however it be deformed, the product of the varying lengths AD, AE, always remains constant. If when these points coincide, the distances AE and AD be taken equal to one foot and then the cell be moved again, when AD takes respectively the lengths 1, $\frac{2}{3}$, $\frac{1}{2}$, $\frac{1}{3}$, &c., then AE will be found to assume the lengths 1, $1\frac{1}{2}$, 2, 3, &c., showing that the length of one is so governed by the length of the other that their product must remain constant.

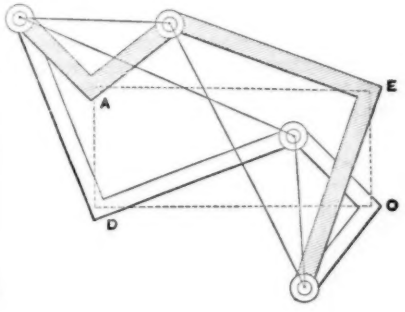


Now Mr. Hart found that if he took four bars and made a linkage in which the adjacent sides are unequal and two cross as in the figure, and then took four points on the four links dividing the distances between the pivots in the same proportion, those points will always remain in a straight line and possess the peculiar property just adverted to, so that the product AD . AE is constant. So also is OE.OD, and also AD.DO and AE.EO. So we see immediately that we may employ Hart's cell of only four bars exactly as we employed Peaucellier's of six bars, and by fixing one of the points as A, and pivoting our extra link to another as D, we can get straight line motion with only five bars, which is the least number possible, as has been absolutely demonstrated.

THE QUADRUPLANE.

A beautiful and important extension of this discovery was made at the same time by Prof. Sylvester and Mr. Kempe. Prof. Sylvester has given quite an elaborate description of it, but I use Mr. Kempe's own words as being simpler. "If we take the contra-parallelogram of Mr. Hart and bend the links at the four points which lie on the same straight line, through the same angle, the four points, instead of lying in the same straight line, will lie at the four angular points of a parallelogram of constant angles—two the angle that the bars are bent through and the other two its supplement—and of constant area, so that the product of two adjacent sides is constant."

If we keep the lettering of the last figure, take the holes or points in the middle of the links and bend them through a right angle as the simplest, we have the figure here given. The four holes now lie at the four corners of a right-angled parallelogram, and the product of any two adjacent sides, as AD.AE, is constant.



It follows that if A be fixed and D pivoted to the extremity of the extra link, whose other extremity is always pivoted to a point equidistant from A and D, the point E will describe a straight line differing in direction from the line it described before the bending by precisely the same angle the bars have been bent through, in this chosen case by a right angle.

By looking at the figure it is seen that the apparatus, which for simplicity has been described as formed of four straight links which are afterwards bent, is really formed of four plane pieces on which appropriate points are chosen. This is why it is called the "Quadruplane" by Prof. Sylvester, who says: "The quadruplane gives the most general and available solution of the problem of exact parallel motion that has been discovered, or that can exist. I say the most available, for it is evident, in general, that piece-work must possess the advantage of greater firmness and steadiness, from the more equal distribution of its strains, over ordinary link-work."

THE PLAGIOGRAPH.

From the ordinary pantagraph familiar to mechanicians, on application of this same idea, namely, turning two of its links into pieces or planes, gives a beautiful extension of it, called by Prof. Sylvester, its inventor, the Plagiograph. "Like the pantagraph, it will enlarge or reduce figures; but it will do more, it will turn them through any required angle." Thus the Plagiograph enables us to apply the principle of angular repetition (as, for instance, in making an ellipse with dimensions either fixed or varying it will, successively turn its axis to all points of the compass), to produce designs of complicated and captivating symmetry from any simple pattern or natural form, such as a flower or sprig. This should be found to place a new and powerful implement in the hand of the pattern-designer and architectural decorator.

ANOTHER IMPORTANT USE.

Finally, we have seen that in using a linkage to draw a straight line, the distance between the fixed pivots must always be the same as the length of the extra link. Now if this distance is not the same, the pencil-point describes, not straight lines, but circles. If the difference be slight, the circles described will be of enormous magnitude, decreasing in size as the difference increases. This property is of very high importance in in the mechanical arts for describing circles of large radius. Prof. Sylvester cites as example some circular steps outside St. Paul's Cathedral, which requiring repair, Mr. Penrose employed a Peaucellier cell to cut out templets in zinc for the purpose. The radius of the steps is about 40 feet; but to the great comfort and delectation of his clerk of the works, they were able to operate with a radius of not more than 6 or 7 feet in length.

These are but the simplest of the innumerable applications contained in, and immediately suggested by, the new science of linkage. Only let the practical mechanician begin to make for himself models of those here described, and we guarantee him a rich harvest of unlooked for results.

In the words of its founder, "I feel a strong persuasion that when the inertia of our operative classes shall have been overcome, this application will prove to be but the signal, the first stroke of the tocsin, of an entire revolution to be wrought in every branch of construction."

It is well for those who manufacture articles liable to decomposition to know that glycerine has the power of arresting fermentation to a remarkable degree. It is stated in the *Chemical Journal* that glycerine retards both lactic and alcoholic fermentations. One-fifth of glycerine added to milk at a temperature of 15 deg. to 20 deg. C. prevents it from turning sour for eight or ten days. One-half or one-third of glycerine, at the same temperature, retarded the fermentation of milk for six or seven weeks. At higher temperatures larger quantities are needed to produce the same results. The formation of hydrocyanic acid from amygdaline and emulsine is also retarded by glycerine. It becomes thus very serviceable in preventing the spoiling of various lotions. For this reason it is not unusual to add a small quantity to the preparation known as milk of roses, and also to almond paste. With regard to cosmetics, generally, the use of glycerine in small quantities may be recommended.

ON AERONAUTICS.

By RICHARD GERNER, M. E.

Written for Van Nostrand's Engineering Magazine.

Long before the locomotive and the steamship were thought of, man cast his eyes longingly over the vast expanse of atmosphere above him, and thirsted after the simple ability which a bird acquires so quickly, and which mankind, after centuries of study and experiments, has not even approximated to. Ovid has told us the tale of the feat of Daedalus in so natural a manner that we should love to think of it as a reality and it drives us on to further thought and experiment. Archytas is said to have constructed a flying dove, but we are sorry to opine that this must be classed among the legends and traditions rather than the facts which have come down to us from those days. There is but one possible means of rising into and traversing through the air faster than a bird, as a crusty but not humorless German professor informed us in 1812, and that is by means of our thoughts, and this too, after having led us through a work of 600 pages descriptive of aeronautical experiments and apparatus, which is all very fine but hardly satisfactory.

Since then, as many years have passed away as there are elements, and we are to this day as unable to go to China by any other means than land or sea as we were then. But is it really true that this sixty-five years long study and research has been to no purpose? Have we not even a clew towards the desired purpose to be effected?

Let us see what has been done in all this time; how the difficulties of the problem of aeronautics have been met and treated, and how far man failed and how far he has been successful.

Primarily, it was desired to produce a means of rising into the atmosphere. And so far as this is concerned, the human mind and ingenuity has experienced a triumph which will be as lasting as it has been successful.

But, paradoxical as it may seem, this success has been the means of delaying the progress of the actual science of aeronautics to a remarkable degree, as the popular mind has become engrafted with the idea that the art and science of ballooning would ultimately and inevitably lead to the solution of the problem. That this is not the case, we shall learn from an examination of the history, construction, principles and results arrived at by the balloon.

The Montgolfier Brothers are generally and popularly accredited with the invention of the balloon, and in so far as they were the first to construct such a thing they are not undeserving of the credit. But Prof. Charles, the Parisian physicist, invented and constructed a hydrogen balloon quite independently of them, and this has not been superseded to this day, while the hot air balloons of the Montgolfiers went out of practice a comparatively short time after their introduction.

The way the Montgolfiers got at their balloon, was as follows: At Annonay, in Vivarrais, not far distant from the very base of the Alps, they owned a paper mill, and here they had the daily opportunity of watching the formation of the clouds on the mountain slopes and then rising into the air. Both were scientifically educated; they often conversed over the causes of the flight of the clouds, and presently the thought occurred to them to imitate this natural phenomenon. But their experiments were a series of sad failures until Priestley's work on different classes of air and gases fell into their hands, wherein they found the possibility of the existence of gases, much lighter than air, discussed. It was only a question of enclosing such gases in a light envelope, but all trials to effect this with paper, failed.

After many vain experiments, they at last, in 1782, arrived at the desired result, but curiously enough, on premises which were utterly ridiculous. Their idea was that one of the principal causes why clouds arise in the air and there remain at rest, or are wafted about without falling to the earth, is electricity. Accordingly, they sought the production of a gas gifted with electric properties, and this production they thought to

effect by mixing gas of alkaline properties with non-alkaline. To this end, they burned straw and an organic substance, like wool, which was to produce the alkaline gases, under a square paper balloon of about forty cubic feet capacity, and to their delight, the balloon arose to the ceiling of the room. That it was simply the heating of the air in the balloon which effected its rising they hadn't the slightest idea. Instead, they thought to have discovered a new gas with remarkable properties and gained many followers, until Saussure, in the following year, terminated the bitter controversy which had arisen, by performing the simple experiment of inflating a small paper balloon by carefully inserting a red hot iron into it, and causing it to rise.

The great desideratum had been arrived at, and now it only remained to carry the thing into practical execution, and accordingly, the Montgolfiers built, in the same year, an apparatus of a diameter of 38 feet, which weighed 450 lbs., and carried an additional weight of 400 lbs., and on the 4th of June, 1783, this airship ascended from a public square in Annonay, to the amazement of the entire inhabitants of Vivarrais. The balloon was constructed of linen pieces simply put together by means of buttons and buttonholes, lined with paper and covered with a string net-work. And on a wire gauze under the opening, ten pounds of straw and wool were burned. Unfortunately, the spectacle only lasted ten minutes, the balloon having risen 1960 feet, and horizontally carried along 7200 feet.

The corporation and inhabitants of Paris received the news of this exhibition, and, as is usual with that capital, went wild over it. The Academy of Science extended an invitation to the Montgolfiers to come and repeat the show. But the excitement was too great to await their coming, and within a few days, 10,000 francs had been subscribed, and Prof. Charles, the favorite physical scientist of the day, an energetic young man, was commissioned to spend this money in preparing a balloon sensation for the excited Parisians.

But Prof. Charles didn't treat the matter in the light of a public amusement. In speculations over the Montgolfiers' mysterious electric gas he didn't lose any time, but applied himself with energy to the feasibility of the employment of hydrogen for the filling of the balloon. Hydrogen was but little known then, and the idea of operating with something like 1000 cubic feet of this dangerous gas, was an appalling one. However, Charles went to work fearlessly and with a will, and the Robert Brothers, who were clever mechanicians, filled his order for a balloon constructed of fine silk in a short space of time, finishing the same Aug. 23, 1783. This huge bubble was filled, on plans entirely original, by air of a barrel serving for the taking up of the iron and water used for the generation of the hydrogen, two tubes leading through holes cut into the head, one into the interior of the balloon, and the other for the introduction of the sulphuric acid. This rude apparatus brought up many difficulties, which threatened the failure of the undertaking. The heat generated by the action of the acid upon the iron, converted a large amount of water into steam, which entered the balloon with the gas and there condensed. Then, also, sulphureted hydrogen, finding an entrance into the balloon, and dissolving in the water formed on the interior of the envelope, might prove fatal in attacking the light fabric. It was necessary, furthermore, to direct streams of water on the balloon to cool it off. It took four days to fill a space of 943 cubic feet about two-thirds full, and 1000 pounds of iron and 500 of sulphuric acid, to produce the 35.75 of hydrogen necessary. But of this 31.75 were lost.

On the 27th of August, at 5 P.M., this balloon arose over the heads of 300,000 spectators assembled in the pouring rain on the Champ de Mars. It maintained a respectable height for about three quarters of an hour and then fell to the ground at Econe, containing a huge rent, owing to Robert having inflated it too much; and in the upper regions, where the air is lighter, the gas in the balloon of course expanded and burst its flimsy shell. This balloon was greeted by the peasants as a huge monster and hunted to death with pitchforks and fire-arms amidst the wildest excitement.

Whence we see that Prof. Charles is

quite as much entitled to the honor of the invention of the balloon as the Montgolfier Brothers are.

On the 1st of December, 1783, he and Robert made an ascent, and he was the second human being that had ever risen above the level of the highest peaks on earth. The first was Pilatre de Rozier, on the 21st of November, but as Charles had published his intent already on the 28th of September, before Rozier had thought of so doing, we must also give him some credit herein. Rozier's ascent was made in a clumsy balloon, 63 feet high, of a diameter of 51 feet, and was of Montgolfier's manufacture. He met with his death, the penalty of his aeronautical intrepidity in 1785; the first victim of the balloon. Charles' balloon had had a capacity of 9200 cubic feet, and had been 26 feet in diameter. Assuming its filling at 6000 cubic feet, the gas weighs 64.5 lbs., taking the moisture into consideration, while 6000 cubic feet of air weigh 516 lbs. The difference is, therefore, 451.5 lbs. As much less than this figure which the balloon, with all its accompanying paraphanalia, weighs, so much will it be capable of carrying into the bargain. Had the same balloon been filled with illuminating gas, this difference would have been 387 lbs.

Europe now began to indulge in the wildest speculations, which ended, unhappily, for the time being, in smoke. The excitement passed over like so many others had done before them and will do after them; many had lost their fortunes and peace of mind in the pursuit of the subject, and a clever few had become millionaires.

Since then, the art, if not the science, of ballooning has become greatly extended, and over 10,000 ascents have been made, of which the celebrated English balloonist, Greene, towards the end of the year 1849, completed 365. Of 1500 aeronants, but 12 have met with an untimely death.

The ascent which Gay-Lussac made in 1804 was the most remarkable for the facts with which it has enriched science, and for the immense height of 23,000 feet above the level of the sea which he attained. At this height, the barometer descended to 12.6 inches, and the thermometer, which was 1° C. on the ground, was 9° below zero. In these regions, the dryness was such on the day of Gay-Lussac's ascent, that hygrometric substances, such as paper, parchment, &c., became dried and crumpled as if they had been placed near the fire. The respiration and circulation of the blood were accelerated in consequence of the great rarefaction of the air. Gay-Lussac's pulse made 120 pulsations in a minute, instead of the normal number of 63. At this great height, the sky had a very dark blue tint, and an absolute silence prevailed. Rozier before him had also made ascents for scientific purposes, but with no recordworthy results.

One of the most remarkable of ascents was made by Mr. Glaisher and Mr. Corewell, Sept. 5, 1861, in a large balloon belonging to the latter. This was filled with 90,000 cubic feet of coal gas, the weight of the load being 600 lbs. After 1 hour and 28 minutes, they had reached a height of 15,750 feet, and in eleven minutes after, a height of 21,000 feet, the temperature being 10.4° C. below zero; another eleven minutes, and they were 26,200 feet high, with the thermometer at 15.2° C. below zero; still another two minutes, and the height attained was 29,000 feet, and the temperature 16° C. below zero. At this height, the rarefaction of the air was so great, and the cold so intense that Mr. Glaisher fainted, and could no longer observe. According to an approximate estimation, the lowest barometric height they attained was 7 inches, which would correspond to an elevation of 36,000 to 37,000 feet.

We have seen that the use of hot air has given way to that of hydrogen, and the latter, in many cases, to that of coal gas, which is preferred on account of its being cheaper and more easily obtained. A balloon of the ordinary dimensions, which can carry three persons, is about 16 yards high, 12 yards in diameter, and its volume about 680 cubic yards; with its accessories, it weighs about 300 lbs., and alone, about two thirds of that amount. The gas is passed into the balloon from the reservoir by means of a flexible tube. The balloon must not be filled quite full, as the atmospheric pressure diminishes as it rises, and the gas inside expanding in consequence of its elastic force, tends to burst it, as it

did in the case of Charles' first balloon. It is sufficient for the ascent if the weight of the displaced air exceeds that of the balloon by 8 or 10 lbs.

The rising and falling of the balloon is easy enough, and if it had not been long proved by direct experiment, Jules-Verne has done it for us to our complete theoretical satisfaction in his interesting work entitled "Five Weeks in a Balloon."

The aeronaut can tell whether he is ascending or descending, either by the barometer or by a long streamer attached to the car. The ascent is effected by throwing out the ballast of sand bags as the occasion requires, and the descent, by the opening of the safety valve on the top of the balloon which allows part of the gas to escape. In so doing, the aeronaut must bear in mind that he is sustaining an irreparable loss, and be careful how he expends the precious means.

As far as the horizontal motion of the balloon is concerned, that is beyond the power or desirability of the aeronaut; he becomes the plaything of the winds, attaining a velocity of from 66.66 to 116. 66 feet per second. Garnerin and Capt. Sowdon, in 1802, on their trip from London to Colchester, in one hour completed 17.5 geographical miles, and Robertson, at Hamburg, about ten. The colossal balloon, which, decorated with 3000 colored lamps and a richly gilded crown, was liberated from the Place Notre Dame de Paris, in Paris, at 11 P.M., Dec. 4, 1804, in honor of the crowning of Napoleon, hovered over Rome at daybreak. Who will bridle such a velocity?

The only practical application which the balloon has experienced is in military reconnoitering, and this has been effected with great success at the battle of Fleurus, in 1794, at Solferino and more lately in the Franco-Prussian war.

And that is what has been done in 65 years, as far as the art and science of ballooning proper is concerned.

In addition, however, much more has been done, and as nearly much more to no purpose. The wildest and most improbable propositions have been advanced, and many have attempted to put these into practical operation. The difficulties, both practical and theoretical, are innumerable and overwhelming, whole libraries have been written on the subject, not a year passes by without adding to the literature already at hand, fortunes have been spent in the construction of designs and the carrying out of vague experiments, and that same Champ de Mars which witnessed the ascent of the first hydrogen balloon, has since witnessed countless failures, and on every one of these occasions, the unhappy apparatus has been ruthlessly destroyed by the mob to satiate its disappointment. There was Jacob Degen, a Viennese horologist, who, in 1812, received a good licking at the hands of a crowd for the failure of his plan; and then there was Lennox, who, in 1834, exhibited his notorious air-ship, the "Eagle," 160 feet high by 48 broad, by 63 feet long, capable of carrying 17 persons, in Paris, which was broken into a thousand pieces by the infuriated spectators. And still we are bid not to despair.

The trouble has been that the projectors of these flying machines have entirely ignored the voice of science; as soon as an idea would strike them, without stopping to enquire into its theoretical correctness, they would immediately plunge into the execution of their improvable schemes without a moment's deliberation, and the necessary result was failure.

The balloon has long been abandoned by scientific men as the foundation to the solution of the knotty problem. The most advanced thinkers have turned their thoughts in an opposite direction, and have come to regard flying creatures, which are all much heavier than atmospheric air, as the true models for flying machines. An old doctrine is more readily assailed than uprooted, and, accordingly, we find the followers of the new faith met by the assertion that insects and birds have large air cavities in their interior, that these cavities contain heated air, and that this heated air, in some mysterious manner, contributes to, if it does not actually produce, flight. No argument could be more fallacious. Many admirable fliers, such as the bats, have no air-cells, while many birds, like the apteryx, and several animals never intended to fly, like the orang-outang, and a large number of fishes are provided with them. It may, therefore, be reasonably concluded that flight is in no

way or manner connected with air-cells, and the best proof that can be adduced is to be found in the fact that it can be performed to perfection in their absence.

According to Dr. I. Bell-Pettigrew, the author of the celebrated work on "Animal Locomotion," and the scientist who was among the first of his time to point out the road to the true solution of the question of aeronautics, there are five primary causes on which all attempts have hitherto wrecked:

First.—The extreme difficulty of the problem. This very cause has given an attractive and fascinating air to the problem, and has hitherto prevented its calm deliberation.

Secondly.—The incapacity or theoretical tendencies of those who have devoted themselves to its elucidation. This cause is now happily eliminated, and like the first, will cease to come into consideration under the earnest application of their thought and time of men like Dr. Pettigrew to the subject.

Thirdly.—The great rapidity with which wings, especially insect wings, are made to vibrate, and the difficulty experienced in analyzing their movements.

Fourthly.—The great weight of all flying things, when compared with a corresponding volume of air. This difficulty will fade more and more as the aforementioned one is eliminated by patient study.

Fifthly.—As we have already stated in a former part of this paper, the discovery of the balloon, which has retarded the science of aeronautics, by misleading men's minds and causing them to look for a solution of the problem in the employment of a machine lighter than the air, and which has no analogue in nature. But it should be remembered, before condemning this circumstance as a difficulty, that the tendency of the new faith may be as erroneous in the end as that of the balloon, and that we have not lost so much after all, by wasting our time on the balloon in seeking for our solution, as we have thereby eliminated a factor from our equation, so to speak, which might have given us no little difficulty in the prosecution of so interesting, important and so complex a subject.

It should also be remembered that past experience has taught us that the genius of the inventor has been quite as important an element in the engineering institutions of the past as the research of the scientist, but, of course, the former is dependent in a great degree upon the latter, and as the scope of that research progresses and enlarges, so do the inventor's genius open new avenues of probable success. It is surprising how much the happy thoughts of the illiterate have contributed towards the progress of engineering and industry.

So we find that if we can trust the new faith, *i. e.*, the solution of the problem by animal flight, that the third difficulty aforementioned is the only practically remaining one. That we may trust in the new faith, such men as Dr. Pettigrew heartily and enthusiastically assure us.

The past trouble with the new faith was that it has been cultivated, on the one hand, by profound thinkers, who have never subjected their theories to experiments, and, on the other hand, by uneducated charlatans who have never subjected their experiments to scientific theory.

There remain many eminent men who still advocate the employment of a machine specifically lighter than air, whom we may style the balloonists; but the ideas which they advance have mostly been practically executed and found to be absurd. They reason that the first consideration is to raise the flying-machine, as it is to make a ship or locomotive go, and that the second consideration is to control this motion. And that is where they are fundamentally wrong, as the question cannot be treated similarly to locomotion on land and sea; and besides, a hundred examples have taught us the fallacy of their reasoning.

We must abandon the balloon altogether, as we have endeavored to show.

But the balloonists do not formulate the only irrational school; a second modern one is that section of the one believing that weight is necessary to flight, which advocates the employment of rigid inclined planes driven forward in a straight line, or revolving planes, *i. e.*, aerial screws.

The other section is more rational, and most likely the right one, trusting for elevation and propulsion to the flapping of wings. This section may be further subdivided into advocates of the vertical

flapping of wings, such as Borelli, Marey and others, and advocates of the partially horizontal flapping of wings, such as Bell-Pettigrew. The favorite idea of the disciples of the inclined plane scheme is the wedging forward of a rigid inclined plane upon the air. It may be made to advance either in a horizontal line, or made to rotate in the form of a screw, whence we also have this section subdivided, and both divisions have their adherents. The one recommends a large supporting area extending on either side of the weight to be elevated, the surface of the supporting area making a very slight angle with the horizon, and the whole being wedged forward by the action of vertical screw propellers. This was the plan suggested by Henson and Stringfellow. The former designed his his ærostat or flying machine, in 1843, and the latter, on Wengham's plan, exhibited his design at the Aeronautical Society's Exhibition, held at the Crystal Palace, London, in the summer of 1868. These formidable and scientific-looking things were never coerced into giving an exhibition of their pretended capacities, and it were therefore useless to consider them.

The first to apply the aerial screw to the air was Sir George Cayley, who, in 1796, constructed a small machine consisting of two corks fastened on either end of a vertical spindle, to the lower part of which is suitably secured the middle of a whalebone bow. To either end of the latter are attached strings which wind about the spindle, and thereby stretch the bow. In the corks are inserted a number of wing feathers from any bird, so as to be slightly inclined, like the sails of a windmill, but in opposite directions in each set. This instrument, after being wound up, readily rises in the air. Sir Cayley calculated that if the area of the screw was increased to 200 square feet, and moved by a man, it would elevate him. But it appears that he never tried it.

This model was immediately seized upon as the basis for a flying machine by a great many people. In 1842, Mr. Phillips succeeded in elevating, by means of revolving fans; a model made entirely of metal, and which, when complete and charged, weighed two pounds. The fans were inclined to the horizon at an angle of 20°, and through the arms the steam rushed, on the principle discovered by Hero, causing the fans to revolve with great energy, so much so that the model rose to a great altitude, and flew across two fields before it alighted. The motive power employed in this instance was obtained from the combustion of charcoal, nitre, and gypsum. This is the first machine that steam ever raised into the air.

The French also seized upon the screw scheme with avidity, and Nadar, Pontin, d'Amecourt and de la Landelle, between the years 1853 and 1863, succeeded in constructing clockwork models, which not only raised themselves into the air, but also carried a certain amount of freight.

It will be readily understood that there is nothing gained by all these machines, and that they are even less efficient than the balloon, and much more costly. What if you can rise into the air with them, and ever so high at that? That is not the question; the question of rising in the air has been solved by the balloon; what we want is direction, and not elevation; the aerial screw is no more governable in this regard than is the balloon.

Whence it appears that we must reject the doctrines of the inclined plane school quite as much as those of the balloonists; and another important and troublesome factor has been eliminated from our equation. Let us see how soon we can get it down to "x equals to."

There now remains to be regarded the doctrines of those who believe in the flappings of wings, to secure the desideratum.

In 1860, Borelli published at Rome a two-volume work, "De Motu Animalium," and up to 1865, all the knowledge that we possessed on the subject is due to this distinguished physiologist and mathematician. He constructed an artificial bird in which the wing, consisting of a rigid spine, with natural feathers attached thereto, flapped vertically downwards, and this idea has been enthusiastically seconded by both Straus-Durckheim and Girard, and quite lately by Professor Marey.

Borelli opines that flight results from the application of an inclined plane, which beats the air, and he evolves,

amongst others, the following propositions from his arguments:

First—If the air strikes the under surface of the wing perpendicularly in a direction from below upwards, the flexible portion of the wing will yield in an upward direction, and form a wedge with its neighbor.

Secondly—Similarly and conversely, if the wing strikes the air perpendicularly from above, the posterior and flexible portion of the wing will yield and be forced in an upward direction.

Thirdly—That this upward yielding of the posterior or flexible margin of the wing results in and necessitates a horizontal transference of the body of the bird.

Fourthly—That to sustain a bird in the air the wings must strike vertically downwards, as this is the direction in which a heavy body, if left to itself, would fall.

Fifthly.—That to propel the bird in a horizontal direction, the wings must descend in a perpendicular direction, and the posterior or flexible portions of the wing yield in an upward direction, and in such a manner as virtually to communicate an oblique action to them.

Sixthly.—That the feathers of the wing are bent in an upward direction when the wing descends, the upward bending of the elastic feathers contributing to the horizontal travel of the body of the bird.

These arguments appear so plausible as to be acceptable to the superficial reader, and even to the philosophers of the past two centuries they have seemed correct in general. Many have changed his plans in detail and proclaimed their new discoveries to the world without giving Borelli credit for the same, and up to this date they have stood firm. The best proof of their invalidity lies in the unfortunate circumstance that they have never succeeded when applied to practice.

Prof. Owen, Macgillivray, Bishop, Liais and others, have added the word *backwards* to Borelli's *downwards.*

Bell-Pettigrew was the first to differ from Borelli and his votaries. He proves that the action of the wing is not downwards and backwards, but downwards and forwards, and that the other arguments stated are fallacious throughout. The artificial wings which he made of late differ from those recommended by Borelli and others in the mode of construction, in the manner in which they are applied to the air, in the nature of the power employed, and in the opinion of the necessity for adapting certain elastic substances to the root of the wing if in one piece, and to the root and the body of the wing if in several pieces.

He maintains that no part of the wing should be rigid; that, if the wing be in one piece, it should be made to vibrate obliquely and more or less horizontally, so as to twist and untwist and make figure-of-8 curves during its action, thus enabling it to seize and let go the air with wonderful rapidity, and in such a manner as to avoid dead points; that the entire wing must be under thorough control during a cycle of motion, and that steam, varying in intensity at every stage of the down and up-strokes, produced by a direct piston action, is the proper motive power; and that the root of artificial wings must be supplied with elastic structures in imitation of the muscles and elastic ligaments of flying animals.

The propounder of what has here been so very briefly referred to has not only the highest faith in his being the true method, by pointing to an early consummation of his plans, but ably and scientifically enters into the merits and minutiæ of his every assertion. His views are sustained by many eminent authorities, who predict its practical success; and we truly believe that the inventor's only chance in this direction is to study Bell-Pettigrew's propositions, ponder them over critically and make them the basis of his speculations and work.

But it must not be imagined that ballooning and aerial animal locomotion are the only foundations upon which both profound philosophers and hair-brained visionaries have built their plans and experiments.

Attempts have been made to harness trained eagles to balloons and other apparatus, and for a long time this possible solution of the question was agitated with fervor and enthusiasm. That this is not the ultimately correct solution is proved by the readiness with which it

was suffered to drop out of notice. A blunt, but well-meaning, individual in a technological journal lately remarked that if humanity couldn't produce any better than animal power to settle its engineering difficulties, it had better resume the furs and bone spears of its barbarous ancestry and give up civilization as a bad job. And we cannot help feeling as he does.

Of course, electricity has been suggested. We noticed a communication from an Australian in the New York *Herald* lately, who had a plan of aerial navigation on electric principles, and only wanted some cash to show the world that his principles could be carried into successful execution. Electricity, somehow or other, can do anything; it is one of these grand, mysterious institutions that will be the future foundation of not only engineering, but of everything. Verne runs and lights his "Nautilus" with it, and this Australian is going to aero-Nautilus it on the same plan. People expect great things from electricity, especially since we can hear the grass grow in Philadelphia with it from New York, and perform other startling feats. People look knowing and hint at future immensities of achievement; the unknown is always what people know most about; ask an average man to extract a square root, to solve an equation of the second degree, or to perform some similar elementary operation, and he'll scratch his head and tell you that he isn't up in that sort of thing; but ask that same man about the future electricity and it is wonderful how much he knows about it, while the sages of all ages and parts of the globe are devoting their life-times to the study of its nature, and finally declare that they don't know anything about it. Ask a professor of mathematics what force is. He don't know. Ask a precocious student. Oh, he knows, and he'll tell you all about it; dealing in arguments and with propositions which are too profound for anybody to understand.

We cannot be too emphatic in warning the precocious inventor against attempting to overreach science. Experience has taught us that it leads to nothing. We do not mean to say that speculation should be abandoned, but we do not believe in building on a foundation which cannot be supported.

Bell-Pettegrew has given us a foundation which will stand. Build on that. Experiment on electricity if you will, don't build on deductions before a critical, scientific community has given them the stamp of validity.

TRANSMISSION OF POWER BY COMPRESSED AIR.

By ROBERT ZAHNER, M. E.

Contributed to VAN NOSTRAND'S MAGAZINE.

I.

HISTORICAL NOTICE.

The application of compressed air to industrial purposes dates from the close of the last century. Long before this, indeed, we find isolated attempts made to apply it in a variety of ways; but its final success must be ascribed to the present age—the age of mechanic arts—an age inaugurated in so splendid a manner by the genius of Watt, and which has been so wonderfully productive in good to mankind.

Without going into any details as to its history, we shall only name the English engineers, Cubitt and Brunell, who, in 1851–4, first applied compressed air in its statical application to the sinking of bridge caissons, the Genoese Professor, M. Collodon, who, in 1852, first conceived and suggested the idea of employing it in the proposed tunneling of the Alps; and, finally, the distinguished French engineer, Lommeiller, who first practically realized and applied Collodon's idea in the boring of the Mt. Cenis Tunnel.

II.

ITS APPLICATIONS AND ITS FUTURE.

The applications of compressed air are very numerous, its most important one

being the transmission of power by its means.

Custom has confined the term "transmission of power" to such devices as are employed to convey power from one place to another, without including organized machines through which it is directly applied to the performance of work.

Power is transmitted by means of shafts, belts, friction-wheels, gearing, wire-rope, and by water, steam and air. There is nothing of equal importance connected with mechanical engineering in regard to which there exists a greater diversity of opinion, or in which there is a greater diversity of practice, than in the means of transmitting power. Yet in every case it may be assumed that some particular plan is better than any other, and that plan can be best determined by studying, first, the principles of the different modes of transmission and their adaptation to the special conditions that exist; and, secondly, precedents and examples.

For transmitting power to great distances, shafts, belts, friction-wheels and gearing are clearly out of the question. The practical incompressibility and want of elasticity of water, renders the hydraulic method unfit for transmitting regularly a constant amount of power; it can be used to advantage only where motive power, acting continuously, is to be accumulated and applied at intervals, as for raising weights, operating punches, compressive forging and other work of an intermittent character, requiring a great force acting through a small distance.

Whether steam, air or wire-rope is to be made the means of transmitting power from the prime-mover to the machine, depends entirely upon the special conditions of each case. In carrying steam to great distances very importannt losses occur from condensation in the pipes; especially during cold weather. The wear and tear of cables lessen the advantages of the telodynamic transmission; steep inclinations and frequent changes of direction of the line of transmission often exclude its adoption; while it is entirely excluded when it is rather a question of distributing a small force over a large number of points than of concentrating a large force at one or two points.

Compressed air is the only general mode of transmitting power; the only one that is always and in every case possible, no matter how great the distance nor how the power is to be distributed and applied. No doubt as a means of utilizing distant, yet hitherto unavailable sources of power, the importance of this medium can hardly be overestimated.

But compressed air is also a *storer* of power, for we can accumulate any desired pressure in a reservoir situated at any distance from the source, and draw upon this store of energy at any time; which is not possible either in the case of steam, water or wire-rope.

Larger supply-pipes are required for steam or water transmission; the inconveniences resulting from hot steam pipes, the leakages in water pipes, the high velocities required in telodynamic transmission are all without their counterparts in compressed air transmission. Compressed air is furthermore independent of differences of level between the source of power and its points of application, and is perfectly applicable no matter how winding and broken the path of transmission.

But especially is compressed air adapted to underground work. Steam is here entirely excluded, for the confined character of the situation and the difficulty of providing an adequate ventilation, render its use impossible; compressed air, besides being free from the objectionable features of steam, possesses properties that render its employment conducive to coolness and purity in the atmosphere into which it is exhausted. The boring of such tunnels as the Mt. Cenis and St. Gothard would have been impossible without it. Its easy conveyance to any point of the underground workings; its ready application at any point; the improvement it produces in the ventilating currents; the complete absence of heat in the conducting pipes; the ease with which it is distributed when it is necessary to employ many machines whose positions are daily changing, such as hauling engines, coal-cutting machines and portable rock-drills; these, and many other advantages, when contrasted with steam under like conditions, give compressed a value which the engineer will fully appreciate.

There is every reason to believe that

compressed air is to receive a still more extensive application. The diminished cost of motive power when generated on a large scale, when compared with that of a number of separate steam engines and boilers distributed over manufacturing districts, and the expense and danger of maintaining an independent steam power for each separate establishment where power is used, are strong reasons for generating and distributing compressed air through mains and pipes laid below the surface of streets in the same way as gas and water are now supplied. Especially in large cities would the benefits of such a system be invaluable; no more disastrous boiler explosions in shops filled with hundreds of working men and women; the danger of fire greatly reduced; a corresponding reduction in insurance rates; an important saving of space; cleanliness, convenience and economy. We say economy! For there is no doubt that a permanently located air-compressing plant, established on a large scale, and designed on principles of true economy and not with reference to cheapnes of construction, would supply power at a much less cost than is supposed. Besides, there are many natural sources of power, as water power, which could by this means be utilized, and their immense stores of energy conveyed to the great centers of business and manufacture.

As affording a means of dispensing with animal power on our street railroads, compressed air has been proposed as the motor to drive our street cars. It has already met with some success in this direction, and, to-day, there are eminent French, English and American engineers at work upon this interesting problem.

The compressed air locomotives of M. Ribourt, now in use at the St. Gothard Tunnel, give very satisfactory results. They are compact, neat and comparatively economical.

Compressed air is also applied in a variety of other ways; in signaling, in propelling torpedo boats; in ventilating large and confined spaces; in driving machinery in confined shops; in sinking bridge caissons. The pneumatic dispatch system, the air brake, the pneumatic elevator and hoist are further examples of its use.

CHAPTER I.

The Conditions Modifying Efficiency in the Use of Compressed Air.

I.

LOSS OF ENERGY.

What is at present required in the use of compressed air is a considerable diminution in the first cost of obtaining it by really improving the compressor, and a practical means of working it at a high rate of expansion without the present attendant losses. In the best machines in use at the present day, the *useful effect*, that is, the ratio of the work done by the air to that done upon it, is very small. The losses are chiefly due to the following causes:

1. The compression of air develops heat; and as the compressed air always cools down to the temperature of the surrounding atmosphere before it is used, the mechanical equivalent of this dissipated heat is work lost.

2. The heat of compression increases the volume of the air, and hence it is necessary to carry the air to a higher pressure in the compressor in order that we may finally have a given volume of air at a given pressure, and at the temperature of the surrounding atmosphere. The work spent in affecting this excess of pressure is work lost.

3. The great cold which results when when air expands against a resistance, forbids expansive working, which is equivalent to saying, forbids the realization of a high degree of efficiency in the use of compressed air.

4. Friction of the air in the pipes, leakage, dead spaces, the resistance offered by the valves, insufficiency of valve-area, for workmanship and slovenly attendance, are all more or less serious causes of loss of power.

The question now is, how can we get rid of these losses and obtain a higher efficiency?

The first cause of loss of work, namely, the heat developed by compression, is entirely unavoidable. The whole of the mechanical energy which the compressor-piston spends upon the air is converted into heat. This heat is dissipated by conduction and radiation, and its mechanical equivalent is work lost. The compressed air, having again reached

thermal equilibrium with the surrounding atmosphere, expands and does work in virtue of its *intrinsic energy*.

We proceed to the second loss, which is the work done in driving the compressor-piston against the increase of pressure due to the heat of compression. Since the temperature increases more rapidly than it ought, according to Boyle's law, the work necessary to compression is greater than if the temperature were to remain constant.

The theoretical efficiency of the compressing and working cylinders, as given further on by eq. (486), is:

$$E=\frac{\theta_0}{T_1},$$

where T_1 is the absolute temperature of the air at its exit from the compressor, and θ_0 the absolute temperature at its entrance into the working cylinder, which in practice is that of the surrounding atmosphere. Hence we can increase the value of this fraction only by decreasing the denominator T_1, that is the final heat of compression. This can only be done by abstracting the heat during compression, or by using very low pressures. But low pressures are excluded by other considerations. The weight of air, w, needed per second to perform a given amount of work would have to be considerably increased, and this would necessitate larger pipes, larger cylinders, and would result in a cumbrous and expensive arrangement.

The only remaining alternative, therefore, is to bring about in the compressor the cooling which the air now undergoes after having left it. Table VII shows respectively the portion of work lost when the air is not cooled in the compressor and that lost when it is completely cooled, and will make manifest the advantage there is in cooling. For a pressure of six atmospheres the work spent in isothermal compression to that spent in adiabatic compression is as 3 to 4; and this ratio decreases rapidly as the pressure increases.

II.

METHODS OF COOLING.

There are three methods in which cold water is applied to cool the air during its compression:

1. In case of the so-called hydraulic piston or plunger compressors, the air is over and in contact with a column of water which acts upon the air like an ordinary piston, its surface rising and falling with the backward and forward motion of the plunger. It is obvious that the cooling effect of this large mass of water is very small. There is nothing but surface contact, and water possesses in a slight degree only, the property of conducting, through its mass, heat received on its surface. But we obtain all the advantages there are in having the air completely saturated with water-vapor during its compression, as well as all the disadvantages of having saturated compressed air to work with. What has been here said of hydraulic plunger-compressors, applies equally to hydraulic or ram compressors (first used by Sommeiller at Mt. Cenis, but now obsolete).

2. By flooding the external of the cylinder, and sometimes also the piston and piston-rod. This method of cooling presents neither the advantages nor disadvantages incident to direct intercontact between the air and water; it is that generally adopted in American practice, especially where it is necessary to expose the air-pipes to the out-door atmosphere of winter. The cooling which it effects is, however, only an approach to that which insures the highest efficiency.

3. By injecting into the compressor cylinder a certain quantity of water in a state of the finest possible division, *i. e.*, in the form of *spray*. This method of cooling was first applied by Prof. Collodon in the compressors used at the St. Gothard Tunnel. It is by far the most rational, complete and effective. In this fine state of division the water has many more points of contact with the air, which is both completely cooled and kept thoroughly saturated during compression. It is extremely important that the quantity of water injected into the compressor be a minimum, and hence the weight required for different tensions is given in a table further on.

III.

CONDITIONS MOST FAVORABLE TO ECONOMY IN THE USE OF COMPRESSED AIR.

By working air at full pressure we avoid the formation of ice in the pipes and exhaust ports, not so much because the air is less cooled (for the great fall

of temperature produced by the sudden expansion at the instant of exhaust is almost equal to that produced by interior expansion), but because the air in exhausting requires a high velocity, and this opposes the deposit of ice crystals by its purely mechanical effect, and by the heat developed by its friction.

But even at full pressure we cannot work with high tensions without serious drawbacks. In England, several trials were made at the Govan Iron Works and other places to use air under tensions of eight and nine atmospheres, but they were forced to return to low pressures, owing to the entire arrest of the machine from the formation of ice in the ports. Hence, not taking into account the fact that the useful effect decreases as the pressure increases, we conclude that it is not good practice, even at full pressure, to work with a tension much over four atmospheres, unless we employ special means to reheat the working air.

But while by working at full pressure with moderate tensions, we avoid the inconveniences of very low temperatures, the efficiency obtained is also very low. Notwithstanding this, even up to the present time air is almost exclusively worked at full pressure, especially in the United States. This is because the great cold produced by expansive working has made its adoption impossible. With a cut-off at $\frac{1}{2}$ stroke the temperature of the air falls 71° C, and at $\frac{1}{8}$ cut off 140° C.

Now, to avoid these low temperatures, is is necessary either that the initial temperature of the compressed air be raised by heating it before its introduction into the working cylinder, or that the cylinder in which it expands be heated, or that the compressed air be supplied with heat directly during its expansion by means of the injection of hot water.

In 1860, M. Sommeiller, in order to utilize expansion, heated his working cylinders at Bardonnéche by means of a current of hot air circulating around the cylinders in small pipes. By this means he was enabled to cut off at $\frac{2}{3}$ stroke.

In 1863, M. Devillez recommended that the cylinder be placed in a tank through which hot water was to circulate. Other devices were to place the cylinder into a tank of water, into which from time to time fresh supplies of quicklime were to be thrown. Waste cotton, soaked in petroleum, was also used to heat the working cylinder.

Finally, in 1874, Mr. C. W. Siemens proposed the injection of hot-water into the compressed-air engine cylinder to keep the temperature of the expanding air from falling below the freezing point, just as we inject cold water into the compressor cylinder to prevent a great rise of temperature during compression. This is by far the most efficient mode of supplying heat to the expanding air. Expansion is made completely practicable, and hence the efficiency of the engine is greatly increased, as was shown by M. Cornet, who was the first to apply Mr. Siemens' plan and to prove conclusively its great practical utility.

The quantities of hot water to be injected into the cylinder should always be a minimum; they are given in a table further on.

IV.

EFFICIENCY ATTAINED IN PRACTICE.

It is desirable to know what efficiencies have been attained in practice—of compressors, of compressed-air engines, and of the two machines together as a system.

1. By efficiency of compressor is meant the ratio of the effective work spent upon the air in the compressor to that developed by the steam in the driving engine; or if you choose the resistance divided by the power.

a. In compressors without piston or plunger, such as the hydraulic compressor of Sommeiller, the efficiency is always less than .50. These machines are interesting on account of their simplicity, but their useful effect is always very small.

b. In the so-called hydraulic piston, or plunger-compressor, an efficiency of .90 has been obtained when working at a low piston-speed to pressures of four and five atmospheres.

c. The compressors of Albert Schacht at Saarbrücken, in which the cooling is wholly external, have shown an efficiency of .80 when compressing to a tension of 4 effective atmospheres.

d. Prof. Collodon's compressors, into which water is injected in the form of spray, and which were run at a piston-speed of 345 feet, and compressed the air to an absolute tension of 8 atmos-

pheres, gave an efficiency which never descended below .80, while the temperature of the air never rose higher than 12 to 15 degrees C.

2. The efficiency of compressed-air engines is the ratio of the work which they actually do to that which is theoretically obtainable from the compressed air. The following are examples of its value as found by experiment:

At the Haigh Colliery, Eng., .70
" " Ryhope " " .66

M. Ribourt has found for his locomotives .50 to .60.

In general it may be said that in the very best machines we can count upon from .70 to .75; while in the ordinary ones, working against a variable resistance, this efficiency descends to .50 and .55.

3. The efficiency of the whole system together, that is, the ratio of the work measured on the crank-shaft of the compressed-air engine, to that done by the prime mover, is found to be about .20 to .25 high pressures, and from .35 to .40 for low pressures.

Experiments made at Leeds show a net efficiency of .255 when working with 2.75 effective atmospheres pressure, and .455 when with 1.33 effective atmospheres pressure.

At the Blanzy mines, M. Graillot has found for a final efficiency, .22 to .32 of the effective work of the steam.

M. Ribourt, by experimenting on the new compressed-air locomotives built for the St. Gothard Tunnel, found that the ratio of the tractive effort developed to the original power, (in this case a head of water), was .23; that is, after passing the turbine, the compressor, the expansion regulator, and the cylinders of the locomotive, there remained .23 of the original power.

V.

THE EFFICIENCY OF FULL PRESSURE AND OF EXPANSION COMPARED.

Let W_1 be the work spent upon the air in the compressor:

W_2 the work which the compressed air is theoretically able to do; then its theoretical efficiency will be $\frac{W_2}{W_1}$.

If W = the actual work done by the prime mover, and

W′ the actual work done by the air, then the real efficiency will be $\frac{W'}{W}$.

Now in the ordinary conditions of practice we know that W_1 is at best .70 W, and W′ is only about .70 W_2; hence

$$E' = \text{real efficiency} = \frac{W'}{W} = \frac{.70 W_2}{\frac{W_1}{.70}} = .49 \frac{W_2}{W_1} = .49E.$$

The value of $\frac{W_2}{W_1}$ (= E = the theoretical efficiency) is .55 for full pressure and .75 for complete expansion. Hence, substituting these values of E above, we find for these two cases a final efficiency of .27 and .37.

VI.

LOSSES OF TRANSMISSION.

The losses due to transmission are calculated further on.

At the works for excavating the Mt. Cenis Tunnel the supply of compressed air was conveyed in cast iron pipes 7⅝ inches in diameter. The loss of pressure and leakage of air, from the supply pipes, in a length of *one mile and fifteen yards*, was only 3½% of the head; the absolute initial pressure was 5.70 atmospheres and it was reduced to 5.50 atmospheres, whilst there was an expenditure at the rate of 64 cubic feet of compressed air per minute. In the middle of the tunnel, through a length of pipe of 3.8 miles, the absolute pressure fell only from six atmospheres to 5.7 atmospheres, or to .95 of the original pressure.

At the Hoosac Tunnel the air was carried through an 8-inch pipe from the compressors to the heading, a distance of 7,150 feet, operating six drills, with an average loss of *two* pounds pressure.

CHAPTER II.

THE PHYSICAL PROPERTIES AND LAWS OF AIR.

I.

INTRODUCTORY.

A fluid is *a body incapable of resisting a change of shape.* Fluids are either liquids, vapors or gases. Water may be taken as the type of the first; steam is the type of all vapors, and air of all gases.

Gases are either *coercible* gases, *i. e.*,

such as under ordinary circumstances may be condensed into liquids or even solids, as CO_2; or *permanent* gases, which retain their aëriform state under all ordinary circumstances of temperature and pressure. This distinction is convenient. Air has been condensed, but certainly not under *ordinary* circumstances.

Air then is a *permanent* gas, and may be considered a *perfect fluid;* that is,

1. It is incapable of experiencing a distorting or tangential stress, its molecules offering no resistance to relative displacement among themselves; hence no internal work of displacement need be considered.

2. It has the power of indefinite expansion so as to fill any vessel of whatever shape or size.

3. It exerts an equal pressure upon every point of the walls of the vessel enclosing it.

4. It is of the same density at every point of the space it occupies.

II.

BOYLE'S LAW.

This law states that the temperature being constant, the volume of a gas varies inversely as the pressure, &c., formulated,

$$pv'=p_0v_0 \qquad (1)$$

Where v_0=the volume of a given weight of the gas at freezing temperature and a pressure p_0; and v'=the volume of the same weight of gas at the same temperature and at any pressure p.

Dry air, a mechanical mixture of oxygen and nitrogen, being a permament gas, obeys this law.

III.

THE LAW OF GAY-LUSSAC.

This second law of gases may be stated thus: The volume of a gas under constant pressure expands when raised from the freezing to the boiling temperature, by the same fraction of itself, whatever be the nature of the gas formulated:

$$v=v'(1+a_1t) \qquad (2)$$

It has been found by the careful experiments of M. M. Rudberg, Regnault and Prof. Balfour Stewart and others, that the volume of air at constant pressure expands from 1 to 1.3665 between 0° C. and 100° C. Hence for a variation in temperature of 1° C., the volume varies by .003665 or $\frac{1}{273}$ of the volume which the air occupied at 0° C. and under the assumed constant pressure. In equation (2) the coefficient a_1 is therefore equal to $\frac{1}{273}$.

IV.

BOYLE'S AND GAY-LUSSAC'S LAW.

Combining the equation formulating Boyle's law with that formulating Gay-Lussac's, we obtain,

$$pv=p_0v_0(1+a_1t)=p_0v_0a_1(\frac{1}{a_1}+t);$$

or letting $a=\frac{1}{a_1}=273$, we have

$$pv=\frac{p_0v_0}{a}(a+t)=\mathrm{R}(a+t) \qquad (3)$$

This last equation is a general expression for both Boyle's and Gay-Lussac's law, and completely expresses the relation between temperature volume and pressure.

R is a constant and depends upon the density of the gas. Its value for atmospheric air is determined as follows:

The weight of the standard unit of volume of a substance in any condition is the *specific weight* of that substance in that condition.

The *specific weight* of air, that is to say, the weight of a cubic foot of air at 0°C. and under a pressure of 29.92 inches of mercury, is according to M. Regnault .080728 lbs. avoirdupois.

The *specific volume* of a gas is the volume of unit of weight; it is the reciprocal of the specific weight.

The *specific volume* of air, *i.e.*, the volume in cubic feet of one pound avoirdupois at 0° C. and under the pressure of 29.92 inches mercury is:

$$v_0=\frac{1}{.080728}=12.387 \text{ cubic feet.}$$

Let p_0=2116.4, the mean atmospheric pressure in lbs. per square foot. Then

$$\mathrm{R}=\frac{p_0v_0}{a}=\frac{2116.4\times12.387}{273}=96.0376.$$

V.

ABSOLUTE TEMPERATURE.

Making $t=-273$ in the equation

$$pv=\mathrm{R}(a+t)$$

the second member reduces to zero, and hence

$$pv=o.$$

The distance of the freezing point from the bottom of the tube of an air thermometer is to the distance of the boiling point from the bottom as 1:1,3665. Hence, in the centigrade scale, where the freezing point is marked 0° and the boiling point 100°, the bottom of the tube will be marked—272°.85. The lowest reading of the scale is, therefore, —273°. If this reading could be observed it would imply that the volume of the air had been reduced to nothing. This is evidently a purely theoretical conception, but in dealing with questions relating to gases it is exceedingly convenient to reckon temperatures, not from the freezing point but from the bottom of the tube of an airthermometer. *Absolute zero*, therefore, is marked—273° on the Centrigrade scale (corresponding to —459.°4 on the Fahranheit's scale) and is the temperature at which all molecular motions cease, and the mechanical effect, which we call pressure, and which is due to these motions, becomes zero.

VI.

LAW OF THE PRESSURE, DENSITY AND TEMPERATURE.

Let D_0=the density of a weight w of air at the temperature 0° C. and under the pressure p_0, v_0 being the corresponding volume;

D=its density at pressure p, temperature t, v being its corresponding volume;

D′=its density at temperature 0° C. pressure p and volume V′.

We shall have

$$D=\frac{w}{v},$$

or by taking w=unity,

$$D=\frac{1}{V}, \text{ and } v=\frac{1}{D}.$$

Placing this value of v in equation (1) we get

$$\frac{p}{p_0}=\frac{D'}{D_0}; \qquad (4)$$

that is, the pressure of a gas is proportional to its density.

From (2) we have,

$$\frac{D}{D'}=\frac{1}{1+a't}=\frac{a}{a+t}; \qquad (5)$$

That is, the density of a gas is inversely as its temperature, the latter being reconed from absolute zero.

Combining equations (4) and (5),

$$\frac{D}{D_0}=\frac{p}{p_0}\times\frac{a}{a+t}, \text{ or}$$

$$p=\frac{p_0}{D_0}\times\frac{a+t}{a}D. \qquad (6)$$

But $D=\frac{w}{v}$, and hence

$$pv=\frac{p_0}{D_0}\times\frac{(a+t)}{a}w \qquad (6a)$$

(6) shows that the density of a gas is:

At constant temperature, directly as the pressure;

At constant pressure, inversely as the absolute temperature.

$\frac{p_0}{D_0}$=constant for any given gas. For air $\frac{p_0}{D_0}=\frac{2116.4}{.080728}=26216.43$ (according to Rankine, 26214); this is the height in feet of a column of fluid of density D_0, which produces a pressure p_0 pounds per square foot of surface; letting H be this height, the weight of the column having one square foot for its surface will be D_0H, or

$$D_0H=p_0.$$

If in (6a) we make v=1, we get

$$w=\frac{p}{a+t}\times\frac{Da}{p_0}=\frac{p}{a\times t}\times\frac{1}{R} \qquad (7)$$

which is the weight of unit of volume, or the *specific weight* of air.

Making w=1 in same equation, we have for the volume of unit of weight,

$$v=\frac{p_0}{D_0 a}\times\frac{a+t}{p}=R\frac{a+t}{p} \qquad (8)$$

called the *specific volume*. (7) and (8) are reciprocals of each other.

VII.

THE MEASUREMENT OF HEAT.

Any effect of heat may be used as a means of measuring it, and the quantity of heat required to produce a particular effect is called a thermal unit. It has been found best to take a thermal unit to be the quantity of heat which corresponds to some definite interval of temperature in a definite weight of a particular substance.

Def. A *British Thermal Unit* is the quantity of heat which corresponds to an interval of one degree of Fahrenheit's scale, in the temperature of one pound of pure liquid water at its temperature of greatest density (39° 1 Fahr).

Def. A *Calorie*, or French Thermal Unit, is the quantity of heat which corresponds to the *Centigrade* degree in the temperature of one *kilogram* of pure liquid water, at its temperature of greatest density, (3° 94 C.).

Def. The *Specific Heat* of a body, is the ratio of the quantity of heat required to raise that body one degree, to the quantity required to raise an equal weight of water one degree.

It has been proven for permanent gases, that,

1. The specific heat is constant for any given gas, and is independent of the temperature and pressure;

2. The thermal capacity per unit of volume, is the same for all simple gases when at the same pressure and temperature;

3. The specific heat increases with the temperature, and probably with the pressure, when the gas is brought near the point of liquifaction, and no longer obeys Boyle's law.

The above three conclusions are true of specific heat at *constant volume*, as well as of specific heat at *constant pressure*, as far as regards simple gases and air, (which, being a mechanical mixture, obeys the same laws as simple gas).

It was shown by Laplace, that the specific heat of a gas is different, according as it is maintained at a constant *volume*, or at a constant *pressure*, during the operation of changing its temperature.

The specific heat of gases was independently determined by M. Regnault and Prof. Rankine; experimentally by the former, and theoretically by the latter. Their results agreed exactly, and are those now generally accepted. As given in Watt's Dictionary of Chemistry,

The specific heat at constant pressure is .238

As we shall find farther on, the specific heat at constant volume is .169.

$$\therefore \frac{c}{c'} = \frac{.238}{.169} = 1.40 = r$$

CHAPTER III.

Thermodynamic Principles and Formulas.

I

INTRODUCTORY.

It is well known that the cylinder of an air compressor becomes very hot even at a low piston-speed. This fact brings us face to face with the doctrine of the conversion of energy; for it is the conversion of the visible, mechanical energy of the piston into that other invisible form of energy called heat. Thus we see we are at the very outset confronted with a thermal phenomenon, whose consideration involves the science called thermodynamics. To begin with we had no other but the visible mechanical energy of a moving piston; but very soon sensible heat manifests itself, and this heat can be developed only at the expense of part at least, of the energy of the moving piston.

These phenomena are referable to the two general principles which form the basis of the science of thermodynamics, viz :

1. All forms of energy are convertible.

2. The total energy of a substance or system cannot be altered by the mutual actions of its parts.

* "The conversion of one form of energy into another takes place with as great certainty and absence of waste, and with the same integrity of the elementary magnitude as the more formal conversion of foot-pounds in kilogrammeters." "In the development of the axioms that nothing is by natural means creatable from nothing, and that things are equal to the same thing only which are equal to each other, and in the application to them of empirical laws with reference to the behavior of bodies under the action of heat and mechanical effect" consists chiefly the science of thermodynamics.

The general equation of thermodymamics which expresses the relation between heat and mechanical energy under all circumstances, was arrived at independently in 1849 by Professors Clausius and Rankine. The consequences of

* "History of Dymamical Theory of Heat," by the late Porter Poinier, M.E., in *Popular Science Monthly* for January, 1878.

that equation have since been developed and applied by many distinguished writers.

Of course we shall here confine ourselves to so much only of the Mechanical Theory of Heat as is necessary to an intelligent comprehension of our subject in doing so, and shall follow in outline the treatment given by M. Pochet, in his admirable "*Nouvelle Méchanique Industrielle*," making free use, at the same time, of the works or Zeuner, Rankine and Clausius.

II.

HEAT AND TEMPERATURE.

Heat denotes a motion of particles on a small scale just as the rushing together of a stone and the earth denotes a motion on a large scale, a mass motion. It is due to a vibratory motion impressed upon the molecules of a body. The more rapid the vibrations the more intense the heat. The quantity of heat in a substance could be measured by multiplying the kinetic energy of agitation of a single molecule by the number of molecules in unity of weight, supposing the substance to be homogeneous and the heat uniformly distributed. Thus the thermometer and dynamometer reveal to us phenomena which are in reality identical, and we can establish a measuring unit to which both effects can be referred.

Temperature is the property of a body considered with reference to its power of heating other bodies. It is a function of the variables, volume and pressure, or,

$$t=\rho\ (v, p)$$

that is, all bodies having the same pressure and volume have the same temperature. This is expressed by the differential equation:

$$dt=\left(\frac{dt}{dp}\right)dp+\left(\frac{dt}{dv}\right)dv, \qquad (9)$$

where $\left(\frac{dt}{dp}\right)$ and $\left(\frac{dt}{dv}\right)$ are the partial differential co-efficients, dt in the former denoting the increment of t when, v remaining constant, p alone is increased by dp; and in the latter, the increment received by t when p remaining constant, v is increased by dv; whilst in the first number of the equation, dt represents the *total* increment of t due to the simultaneous reception by p and v of the increments dp and dv, respectively.

III.

THE TWO LAWS OF THERMODYNAMICS.

The whole mechanical theory of heat, rests on two fundamental theories: *

1. That of the equivalence of heat and work; whensoever a body changes its state in producing exterior work, (positive or negative), there is an absorption or disengagement of heat in the proportion of one British thermal unit for every 772 foot pounds of work, (or of one French thermal unit for every 423.55 kilogrammeters of work).

This mechanical equivalent of heat was first *exactly* determined by Mr. Joule, in honor of whom it is called Joule's equivalent, and is denoted by the symbol J.

2. The theorem of the equivalence of transformations; when a body is successively put in communication with two sources of heat, one at a higher temperature t, the other at a lower temperature t_0, its temperature remaining constant and equal to that of each source during the whole time of contact, and the body neither receiving nor losing heat except by reason of its contact with the two sources, the ratio of the quantity of heat Q given out by the higher source to the quantity Q^1 transferred to the lower source, is independent of the nature of the bodies; it depends only on the temperatures, t and t_0, of the two sources.

Clausius states this as follows: In all cases where a quantity of heat is converted into work, and where the body effecting this transformation ultimately returns to its original condition, another quantity of heat must necessarily be transferred from a warmer to a colder body; and the magnitude of the last quantity of heat, in relation to the first, depends only on the temperature of the bodies between which heat passes, and not upon the nature of the body effecting this transformation; or, more briefly, heat cannot *of itself* pass from a colder to a warmer body.

IV.

HEAT AND MECHANICAL ENERGY.

The quantity of heat which must be imparted to a body during its passage, in a given manner, from one condition to another, (any heat withdrawn from the

* See Clausius on Heat, Memoir.

body being counted an important negative quantity) may be divided into three parts, viz :

1. That employed in increasing the heat actually existing in the body;
2. That employed in producing interior work.
3. That employed in producing exterior work.

The first and second parts, called respectively the *thermal* and *ergonal* content* of the body, are independent of the path pursued in the passage of the body from one state to another; hence both parts may be represented by one function, which we know to be completely determined by the initial and final states of the body. The third part, the equivalent of exterior work, can only be determined when the precise manner in which the changes of condition took place is known.

Let dQ=the element of heat absorbed during an infinitesimal change of condition;

U_0=the free heat present in the body at the beginning, *i.e.*, the body's intrinsic energy;

U=the free heat present in the body at the end of the change, plus the heat consumed by internal work during the change of state;

pdv will be the work accompanying the passage of the body from a state (p,v) to a state $(p+dp, v+dv)$;

Then the heat spent while the body passes from one temperature t to another $t+dt$, and from one state (p,v_1) to another $(p+dp, v+dv)$ will be :

$$dQ=(U-U_0)+\frac{1}{J}.pdv,$$

$$=dU+\frac{1}{J}.pdv; \qquad (10)$$

where du depends upon the *initial* and *final* circumstances, while $\frac{1}{J}.pdv$ depends on the intermediate circumstances of the change of state.

We can write $du=o$ and entirely exclude interior work and heat by confining ourselves to *cyclical processes*, that is to say, to operations in which the modifications which the body undergoes are so arranged that the body finally returns exactly to its original condition, the interior work, positive and negative, exactly neutralizing each other.

$$u=f(p, v),$$

that is, the internal heat of a body depends only upon the volume of the body, and the pressure to which it is subjected. Hence the increase of internal heat when the body passes from a state (p, v) to a state $(p+dp, v+dv)$ will be:

$$du=\left(\frac{du}{dp}\right)dp+\left(\frac{du}{dv}\right)dv \qquad (11)$$

Substituting in equation (10) the value of du as given by equation (11), we have

$$dQ=\left(\frac{du}{dp}\right)dp+\left\{\left(\frac{du}{dv}\right)+\frac{p}{J}\right\}dv \qquad (12)$$

an equation which is not integrable; since this would require that the second derivatives of the co-efficients of dp and dv (which are, respectively, $\frac{d^2u}{dp.dv}$ and $\frac{d^2u}{dv.dp}+J$) should be equal to each other*; this would imply the impossible condition $J=o$. That is, mechanically speaking, the quantity of heat passing cannot be expressed as a function of the initial values of p and v. The equation can only be integrated when we have a relation given, by means of which t may be expressed as a function of v, and therefore p as a fnnction of v alone. It is this relation which defines the manner in which the changes of condition take place; the quantity of heat passing depends upon the *intermediate* circumstauces of change of state, circumstances which may be anything.

When a body is heated from a temperature t to another $t+dt$, preserving the *same volume*, no external work will be done and $dv=o$. Hence eq. (12) will become:

$$dQ=\left(\frac{du}{dp}\right)dp$$

$$=c_1\,dt \qquad (13)$$

which, by definition, is the specific heat at constant volume.

The above equation gives:

$$\frac{du}{dp}=c_1\left(\frac{dt}{dp}\right) \qquad (13a)$$

* Clausius on Heat. Memoir.

* See Ray's Infinitesimal Calculus, p. 366; also McCullough on Heat, arts. 61 and 62.

the partial differential co-efficient of t with respect to p.

If the body passes from t to $t+dt$ under *constant pressure*, $dp=o$, and hence (12) becomes:

$$dQ=\left\{\left(\frac{du}{dv}\right)+\frac{p}{J}\right\}dv=c\,dt \qquad (14)$$

which, by definition, is the specific heat at constant pressure.

From (14) we have:

$$\left(\frac{du}{dv}\right)+\frac{p}{J}=c\left(\frac{dt}{dv}\right). \qquad (14a)$$

Substituting these values of the partial derivatives in eq. (12), we obtain a second expression for dQ, viz.:

$$dQ=c_1\left(\frac{dt}{dp}\right)dp+c\left(\frac{dt}{dv}\right)dv \qquad (15)$$

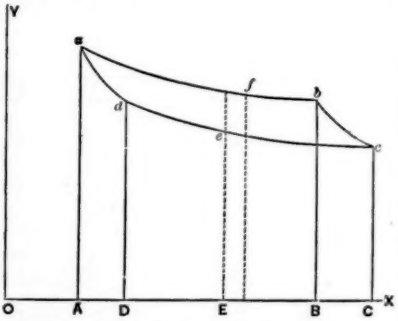
It is convenient to have this equation in a form involving only the temperature and specific heats, and not the quantity Q. We obtain such a form by differentiating (13a) with respect to v, and (14a) with respect to p and subtracting the first result from the second. The form obtained is:

$$\frac{1}{J}=(c-c_1)\frac{d^2t}{dv.dp}+\left(\frac{dc}{dp}\right)\left(\frac{dt}{dv}\right)-\left(\frac{dc_1}{dv}\right)\left(\frac{dt}{dp}\right) \qquad (16)$$

V.

THE DIFFERENTIAL EQUATION OF THE SECOND PRINCIPLE.

In the figure,



1. Let OA=the initial volume of a body whose temperature is t; it expands in contact with a source of heat, (isothermally), from volume OA to volume OB, when its temperature is then still t.

Q=the quantity of heat supplied by the source;

2. It is now left to expand adiabatically, *i.e.*, without the addition or subtraction of heat, from volume OB to volume OC, when its temperature will have fallen to t_0;

3. Now place it in contact with a source of heat of the same temperature t_0, and compress it from OC to OD, when its temperature is still t_0.

Q^1=the quantity of heat that has passed into the source;

4. Compress it adiabatically from volume OD to volume OA, when its temperature will again be t; the body has now undergone a *complete cycle*, during which it has evidently done work represented the area *abcd*; hence,

$Q-Q^1$=heat disappeared, and from the first law of thermodynamics,

$$Q-Q_1=\frac{1}{J}\times abcd=\frac{1}{J}\times A. \qquad (17)$$

Now the second law of thermodynamics states that Q and Q^1, (the heat received and the heat given out), are independent of the nature of the bodies, and dependent only upon the temperature.

Suppose that the difference of temperature of the two sources of heat is infinitely small, t and $t+dt$. Also consider t and v as the independent variables determining the state of the body, $p=f(v, t)$.

A, in the above equation, is the integral between v_0 and v of the elementary areas, such as ef. Now if $Ee=p$, Ef is what p will become when the volume remains constant, and the temperature takes an increment dt; fe therefore measures the differential increment

$$\left(\frac{dp}{dt}\right)dt,$$

where $\frac{dp}{dt}$=the partial derivative of p with respect to t.

$$\text{Hence, } Q-Q'=A.\frac{1}{J}=\frac{1}{J}\int_{v_0}^{v}\left(\frac{dp}{dt}\right)dtdv$$
$$=\frac{1}{J}dt\int_{v^0}^{v}\left(\frac{dp}{dt}\right)dv,$$

taking the independent variable dt out of the integration symbol.

Q is the heat supplied to keep at t the

temperature of the body expanding from v_0 to v, and, therefore,

$Q=\rho(t,v_0,v$; the nature of the bodies);

also,

$$Q'=F'(t,\ v_0,\ v)=F(t),$$

the variables v_0, v being implicitly contained in F.

Since $Q=Q'$ when t becomes $t+dt$ we have,

$$Q=F(t+dt)=F(t)+F'(t)\ dt$$

and

$$\frac{Q}{Q_1}=1+\frac{F'(t)}{F(t)}dt.$$

According to the second principle,

$\frac{Q}{Q'}$ is independent of the nature of the bodies; hence,

$$Q-Q'=Q'\frac{F'(t)}{F(t)}dt=\frac{1}{J}\,dt\int_{v_0}^{v}\left(\frac{dp}{dt}\right)dv$$

and

$$Q'=\frac{1}{J}\frac{F(t)}{F'(t)}\int_{v_0}^{v}\left(\frac{dp}{dt}\right)dv.$$

Now, suppose $v-v_0$ becomes indefinitely small and equal to dv; Q' will become dQ, Q being the heat necessary to keep at t the temperature of a body whose volume increases by dv; hence the differential equation of the first order,

$$dQ=\frac{1}{J}\rho\ (t)\frac{dp}{dt}\,dv \qquad (18)$$

the differential equation of the second principle.*

Calculation of the function $\rho\ (t)$. It may have several forms. Making $dt=o$ in eq. (9), we get,

$$dp=-\frac{\left(\frac{dt}{dv}\right)}{\left(\frac{dt}{dp}\right)}dv;$$

Placing this value of dp in eq. (15),

$$dQ=(c-c_1)\left(\frac{dt}{dv}\right)dv.$$

Moreover in (9) $\left(\frac{dp}{dt}\right)$ represents the partial derivative of p with relation to t when v is constant; making $dv=o$,

$$\left(\frac{dp}{dt}\right)=\frac{1}{\left(\frac{dt}{dp}\right)};$$

Hence eq. (18) may be written,

$$dQ=\frac{1}{J}\rho(t)\frac{dv}{\left(\frac{dt}{dp}\right)}.$$

Equating this with the value of dQ above, we have,

$$\frac{1}{J}\rho\ (t)=(c-c_1)\left(\frac{dt}{dp}\right)\left(\frac{dt}{dv}\right), \qquad (19)$$

from which $\rho\ (t)$ may be calculated.

Again, if we take Eq. (16) and suppose it applied to bodies whose specific heats c and c_1 are independent, the first of the pressure and the second of the volume, as is the case in permanent gases, these conditions give $\left(\frac{dc}{dp}\right)$ and $\left(\frac{dc_1}{dv}\right)$ equal to zero, and the equation becomes,

$$(c-c_1)\left(\frac{d^2t}{dp\,dv}\right)=\frac{1}{J}. \qquad (20)$$

Dividing eq. (19) by this we get,

$$\rho(t)=\frac{\left(\frac{dt}{dp}\right)\left(\frac{dt}{dv}\right)}{\frac{d^2t}{dp\,dv}} \qquad (21)$$

giving $\rho\ (t)$ as a function of t $[=f(p,v)]$ and of its partial derivatives.

Mr. Arnold Hague, the eminent American geologist, has been engaged by the Chinese Government to examine and report upon the mineral resources and mining industry of the Celestial Empire, and sailed from San Francisco on Thursday, the 15th of August, by the steamer Gaelic, to enter upon his duties. He expects to take the field immediately upon arrival, and continue active operations until about the first of December, when he will go into winter quarters. The excellent work performed by Mr. Hague in connection with King's Survey of the Fortieth Parallel, and more recently in Guatemala, is a guarantee of his fidelity and skill in this new undertaking.

* See Zeuner, "Théorie Méchanique de la Chaleur," troisième section, iii.
Also, Clausius on Heat, first Memoir.

RECENT ADVANCES IN THE MANUFACTURE OF IRON AND STEEL, AS ILLUSTRATED IN THE PARIS EXHIBITION.*

By RICHARD AKERMAN, Professor at the School of Mines, Stockholm.

From "The Engineer."

As international exhibitions have of late followed so close on each other, it is natural that the discoveries and inventions that can be made in the interval between each and its successor are not numerous. The technical literature too, especially that which is concerned with the manufacture of iron and steel, has in the last fifteen years been so developed that nearly all improvements are, early after their introduction, found described in a number of periodicals. This has been conspicuously the case since the foundation of the Iron and Steel Institute, which I now have the honor of addressing, and which has been beneficial in so high a degree to that branch of metallurgy to which its attention is more particularly devoted; for at its meetings, as is well known, the most pressing questions affecting the production of iron and steel have been discussed with eminent practical knowledge from every point of view, and many facts highly interesting to the manufacturer, and of which, without intervention of this excellent association, mankind would at most have had but a faint idea, have been, thanks to your "Transactions," disseminated over the whole world. In this connection I must also ask to be allowed to point out another advantage which this association has brought about. Ten years ago there still prevailed at many iron and steel works a very great reluctance to open their doors to strangers, and many an establishment which now willingly admits strangers was then, if not altogether, shut, at least not accessible in the same degree as now. Who can well deny that the opinions expressed by the Institute conducted in a very great degree to bring about this change? And, further, that the facilitated access to iron and steel works has greatly promoted a general knowledge of the latest advances and improvements? A certain result, however, of all this is, that an iron metallurgist, who has properly kept pace with the times, can now scarcely expect that an International Exhibition can produce anything altogether new to him within its walls. Neither for this reason ought it to be required of me, that I should have something new to say to you, even with all the resources of that on the Champs de Mars behind me. Indeed, I would never have entertained the question of making a demand on your precious time, as I now do, if I had not been asked to do so by certain prominent men within this society.

As the leading principle pervading the whole of modern iron manufacture, it must in the first place be pointed out how the cinder-free ingot, iron and steel, is always more and more supplanting the old cinder-mixed wrought iron. This change, as is well known, derives its real origin from the time of Mr. Bessemer's grand invention, which marks an epoch in the history of the iron trade. This important change in the process has also been powerfully assisted by the diminution in the cost of fusing iron and steel, which has been placed within reach by the important application of the so-called regenerative principle by our honored president, Dr. Siemens. For, as we all know, it is not enough that crucible steel can by means of this furnace be made more cheaply, but the Siemens furnace itself has also realised the long-cherished hope of being able, without the help of the costly crucible, to melt steel and iron. Open-hearth metal may be said to have celebrated its baptismal ceremony just at the last Paris Exhibition, when it was named, after its first maker, Martin metal. The Bessemer manufacture, though then ten years old, may be said to have been at the same time in its childhood; and though much railway material of Bessemer metal was shown at that Exhibition, the opinion of its goodness was yet so little established that there were works which, under the common appellation cast steel, sought to conceal that their products were manufactured by the Bessemer process.

* Iron and Steel Institute.

How different is the aspect of affairs to-day, after an interval of only eleven years! Although many a Bessemer works now employs materials inferior to those then used, none seeks any longer to conceal its Bessemer manufacture, but with pride exhibits its Bessemer rails, which, as is well known, are now in process of completely supplanting rails of puddled iron; and one can form some idea of the completeness of the arrangements for rolling Bessemer rails by inspecting the rails from Seraing, 55 metres in length; from Charles Cammell and Co.'s, 43 metres; and Brown, Bayley, and Dixon's rails, 130 feet long, rolled direct from the ingot without intermediate heating. Sweden had, indeed, already, at the Paris Exhibition of 1867, shown the finest razors and other similar wares of Bessemer metal, and in the manufacture of cutlery in Sweden this material is now almost exclusively employed. Styria had likewise then to offer beautiful work of embossed Bessemer metal; but these cases formed at that time rare exceptions, depending on the special goodness of the ores which were employed in the Bessemer manufacture of those countries. For some time Bessemer metal was almost exclusively confined to the manufacture of rails and some other descriptions of railway material. The Exhibition of 1878, on the contrary, affords clear evidence that Bessemer metal is now in most countries employed for purposes for which only a few years ago it was not generally considered sufficiently good. It appears also to have already become very evident that the formerly only too prevailing view that Bessemer metal must necessarily be inferior to other ingot metal only resulted from certain Bessemer works which produced both Bessemer and open-hearth metal, employing for the former more impure materials than for the latter. Where similar materials are used in each case, the ingot metal may be as good from the Bessemer converter as that from other sources. In other words, the quality of the ingot metal is not so much dependent on the methods, Bessemer, Siemens-Martin, or crucible melting, as on the purity of the materials, and the care with which the products are sorted according to their degree of hardness. To sum up here all the purposes for which this Exhibition proves that Bessemer metal has been employed would carry us beyond the compass of this short paper, but it is perhaps right to point out some of them. Thus in the French division, Lobel and Turbot exhibit heavy chains, welded in the common way, made of Bessemer iron from La Société des Forges de Dénain et d'Anzin. In the same way, Ernest Dervaux-Ibled manufactures railway wagon couplings, screw-bolts, and other similar articles of Bessemer iron, from the Bessemer works just named. Further, not only several French makers, such as David, Damoizeau, Doremieux Fils and Cie., and the Société de Commentry Fourchambault, but also Brown, Bayley, and Dixon, of Sheffield, have exhibited heavy Bessemer chains without weld, produced on nearly the same principle as has long been employed for lighter chains, as dog-couplings and such like. La Compagnie des Fonderies, Forges, et Acieries de Saint Etienne exhibits Bessemer rings for cannon. Similar articles, we learn, are also produced at Seraing, whose beautiful display, like several others, as, for instance, those of the Oesterreichische Staats Eisenbahn Gesellschaft in Hungary, and Demidoff in Russia, comprehend good boiler-plate of Bessemer iron. Similar boiler-plate was also exhibited by the West Cumberland Iron and Steel Company, and to give an idea of its good quality, a large hole has, by the help of dynamite, been driven through the middle of the plate without its being possible to see that any portion of the plate has been wrenched away by the violent explosion; for the hole is bounded by edges that have been bent out at right angles, but have not been torn off. Both the evenness and excellent quality of the Bessemer, as well as the Siemens-Martin plate, and the very great superiority of both over plates of puddled iron, are seen most clearly by the exhibit of the Swedish Iron Board (Fercontoret), which shows that the ingot plate, when tested with a falling weight, withstood from five to nine blows from a height of 4.5 metres without the least failure; while the Swedish iron plate only withstood four to six blows of the same weight from a height of only 1·5 metre, or a third of the height in the ingot-plate tests. Further, in these tests, with a falling weight, the buckling be-

fore the least sign of fracture averaged 150 to 160 mm., while the Swedish plate of puddled iron never permitted before fracture greater buckling than 104 mm. Nevertheless, the Swedish iron plate was, as such, of very superior quality, for tests, made with the same falling weight; of best best Staffordshire and best Yorkshire plates showed that the former gave way at the first blow from a height of only 1 metre, while the Yorkshire plate at the utmost withstood three blows from a height of 1.5 metre, and showed in that case a buckling of 68 mm. When the height of fall of only 1.5 metre used for the puddled plates was employed for the ingot plate the latter withstood twenty-five blows, while, on the other hand, the weight at the first blow passed through even the Swedish plate of puddled iron when the fall-height of 4.5 metres used for the ingot plate was also employed for it. Tests were also made for the ingot plate with a fall from a height of up to 9 metres, when it withstood before fracture three blows with the same buckling as in the case of the lower height, also before fracture. Plates of Swedish iron made on the Lancashire hearth, as might have been expected beforehand, appeared in respect to its qualities to lie between those of puddled iron and those of ingots, inasmuch as it was much better than the former, but far inferior to the latter. The ball used as a falling weight in all these tests had a weight of 875 kilogs., spherical in its lower end, and a diameter of 253 mm. The interior diameter of the iron foundation to which the plates were fastened during the tests with thirty-six rivets in a double row was 537 mm. The diameter of the falling weight was thus to the diameter of the part of plate exposed to buckling as 10 to 21. All the plates were 9 mm. thick and 1 metre in diameter.

These experiments, besides, show how enormous is the influence which the content of phosphorus exercises on the power possessed by iron of resisting blows; for the main difference between the chemical composition of the different puddled plates lay in their quantity of phosphorus, for while the Swedish puddled plates contained only 0.016 to 0.021 per cent. of phosphorus the percentage in the Yorkshire plate was 0.094, and in the Staffordshire plate, 0.203. In addition to this difference in the content of phosphorus, there is also in the Staffordshire plate a larger quantity of silicon, or more probably of cinder. No proper difference between Bessemer and Siemens-Martin plates could be discovered in the course of these experiments, which comprehend both complete analyses and tension tests. Yet it almost appears as if the Martin plates have a somewhat greater ductility than Bessemer plate with the same content of carbon. This is also confirmed by the numerous and complete tables of breaking and other tests included in the beautiful exhibit of the Oesterreichische Staats Eisenbahn Gesellschaft. From these it appears to follow that the Bessemer metal made by this company at Reshicza has, in general, a somewhat greater tensile strength, but, at the same time, also less ductility, than Martin metal of corresponding degrees of hardness from the same works. These differences, however, probably depend not so much on the method of production as upon a trifling excess of the contents of phosphorus and silicon in the Bessemer over the Martin metal, made from materials of equally good quality. The Siemens-Martin lends itself more readily than the Bessemer process to the production of large and heavy pieces, inasmuch as there is naturally much less difficulty in simultaneously melting in several large Siemens furnaces, for which no blast is required, that in blowing in at the same time several Bessemer converters. This is also the reason why the Compagnie des Forges et Aciéries de la Marine et des Chemins de Fer, which uses Bessemer metal for its smaller cannon, makes the larger of open-hearth metal. The largest ingot which is to be found in the Exhibition was, probably, from the cause just named, made by the Siemens-Martin process. For Creusot shows in its splendid and well-filled Exhibition pavilion a representation in natural size of an ingot made in this way, weighing 120,000 kilogs. The largest actual ingot which is shown is also made by the same process, and is to be seen in the no less beautiful exhibit of the above-named Compagnie des Forges et Aciéries de la Marine. Siemens-Martin iron is, as is well known, employed to a greater extent than Bessemer for plates, axles, and

other nice purposes, of which also the Exhibition yields such numerous specimens that it is perhaps unnecessary to notice any separate examples. I therefore confine myself to pointing out how, among others, both the above-named works, the Compagnie des Forges et Aciéries de la Marine and des Chemins de Fer and Creusot, use Martin steel for rings and tubes for cannon, and Martin iron for heavy armor plates. John Brown and Co. and Charles Cammel and Co. also exhibit heavy armor plates, consisting partly of ingot iron, for these plates are not exclusively made of it, but consist of about half of puddled and half of ingot iron. The plates are said not to be welded together in the common way of thick puddled and ingot iron laid upon each other, but we learn that the union of the different sorts of iron is brought about at the former works by casting fused iron over a properly-heated puddled iron plate provided with a high iron border, while Cammel makes his double plates by melting down the ingot iron in a furnace whose bottom, so to speak, consists of the puddle iron plate, and then letting them cool together. Both these processes are, of course, finished by rolling. The methods of working just described, as well as the fact before referred to, of Bessemer chains without and with weld, certainly prove the groundlessness of apprehended difficulties in the welding of ingot iron. That heavy armor plates even can be produced of open-hearth metal, by piling and welding together in the way commonly used for puddled iron, is, however, shown by the Compagnie des Forges et Aciéries de la Marine, which, along with its ingot plates, made each of an ingot, also shows an armor-plate 0·56 metre thick, 4·20 metres long, and 1·42 metre broad, weighing 26,500 kilogs. This plate was produced by piling and welding together an anormous number of ingot iron bars. Besides, not only two Swedish exhibits, but also those of the Oesterreichische Staats Eisenbahn Gesellschaft and others afford the clearest evidence that if the ingot metal is only of sufficiently pure quality, it is possible to weld completely, not only the softest qualities, but also very hard Bessemer and Martin metal. The idea of producing armor-plates by piling and welding together ingot iron, instead of making it of a single large ingot, is grounded on the fear that if there be any defect in the ingot, the whole of the plates made from it would thereby be rendered unserviceable, while, on the other hand, when many different layers are welded together, a defect occurring in any of them would not have so great an influence on the plates. The maker of such plates is, in other words, influenced in this point by the same fear which leads to rings for cannon being produced by the welding together of spirals, instead of making them in the common way for tiers by the punching and rolling of an ingot. In the same proportion, however, as greater experience and care lead to greater success being attained in producing more reliable ingots, the more complex method of piling and welding ought to be less frequently used. In any case, the series of experiments on plates above referred to as included in the exhibits of the Swedish Iron Board, are in my opinion so conclusive as to the superiority of the ingot plates over the puddled plates in the case of violent blows, that there can scarcely be any doubt but that soft ingot iron will, in course of time, completely replace puddled iron for armor-plates. The difficulty is to find the right degree of softness and to learn properly to handle the less easily-managed ingot iron. The largest armor-plate which the Paris Exhibition has to offer is of puddled iron, made by Marrel Frères, and has the following dimensions: — Length, 4.250 metres; breadth, 1.600 metre; thickness, 0.715 metre; and weight, 38,022 kilogs. As we have now seen not only how soft ingot steel, but in recent times even soft ingot iron, has begun more and more to take the place of wrought iron, it may not perhaps be out of place to point out in a few words how it has become possible to produce this soft ingot iron which has shown itself to be so superior. There are, indeed, some exceptional Bessemer works, as, for instance, Westanfors in Sweden, where, without any extra addition, the softest iron can be made without its suffering from any red-shortness, and this, as is well known, is more easy of accomplishment in proportion as the pig iron employed contains, on the one hand, more manganese, and, on the other, less sulphur. If a product free from red-

shortness is to be obtained, however, it is in general necessary, at the close, not only of the Bessemer, but also of the Siemens-Martin process, to add an iron more or less rich in manganese, and the quantity of manganese added must indeed be greater in the same proportion as the product is desired to be softer or poorer in carbon. This was the reason why Bessemer and Martin iron of proper softness could only be produced exceptionally until there was a supply of iron compounds very rich in manganese. For as compounds of iron and manganese commonly contain more than 4.5 per cent. of carbon, no great quantity of such a compound can be added, even to the iron poorest in carbon, without the content of carbon in the final product being so great that it ought not to be counted as iron, but as steel. As now, as has been stated, an addition of manganese, the amount of which must be ascertained in every separate case, in order that an ingot metal decarburetted to a certain degree shall be free of red-shortness, it follows that the richer in manganese the added substance is, the less of it requires to be used, and the less carbon accordingly is carried into the final product, or, in other words, it can be made the softer. This was already seen by several persons in the middle and towards the close of the decade 1860-70, and in particular, Mr. Kohn sought by articles in the newspaper *Engineering* to draw the attention of the makers of Bessemer and Siemens-Martin metal to the importance of using the iron compounds then considered rich in manganese, as containg 20 to 30 per cent., which were manufactured by Mr. Henderson at Glasgow in 1866 and 1867. This advice, however, was fruitless, and the manufacture of ferro-manganese soon came to an end from want of demand for the costly product. The matter, however, was soon taken up again by Terrenoire, which, thanks to its eminent engineer, Mr. Walton, understood better than other Bessemer works, the advantages which more manganiferous iron compounds were calculated to confer, and therefore purchased not only Henderson's but also Prieger's patent for the manufacture of ferro-manganese.

Since Terrenoire took the matter in hand the methods of producing this article have been rapidly improved, so that very soon ferro-manganese made in a Siemens furnace with from 50 to 60 per cent. manganese was offered for sale. The process of manufacture was still, however, costly, and the product, therefore, dear. The price, on the other hand, fell rapidly, when by the help of regenerative heating apparatus of the Siemens-Whitwell or Siemens-Cowper systems and very basic charges, success was attained in producing in coke furnaces ferro-manganese compounds, with over 80 per cent. manganese. Of the extension which the manufacture of ferro-manganese in the blast furnace has since undergone, the Exhibition gives a good idea, inasmuch as specimens, with more than 70 per cent. manganese, are shown by so many works that it is, perhaps, unnecessary here to enumerate them. The richest in manganese, with 87 per cent., is, however, made by les hauts fourneaux de Saint Louis, at Marseilles, now the seat of the most extensive manufacture of ferro-manganese. The furnaces under the management of Professor Jordan are, besides, the first which in France began to utilize on a great scale the rich and pure ores in which the coasts of the Mediterranean are so rich, and which have become of so great importance for the French iron manufacture. Besides spiegeleisen and ferro-manganese, there are manufactured here, all with coke, pig for steel for puddling, as well as Bessemer and Martin pig, along with a pig which is employed in competition with charcoal pig in Franche Comté forges, and finally, pig for malleable castings. The supply of ferro-manganese has led to a new method being employed for utilizing old worn-out rails, rich in phosphorus, begun at Terrenoire in 1874, and since very extensively followed. It has been long known that phosphorus has to a certain degree the same influence on the qualities of iron as carbon, inasmuch as both these substances diminish the ductility of the iron, but increase its hardness, modulus of elasticity, tensile strength, and disposition, when heated, to take the crystalline texture, with the resulting difficulty of working at very high temperatures, and brittleness in the cold state. The great difference between the influence of the substances, however, is that the action of carbon is much greater than

that of phosphorus in improving the qualities of iron by increasing its hardness, modulus of elasticity, and tensile strength, while on the other hand the influence of phosphorus far surpasses that of carbon in deteriorating its qualities by increasing the disposition to form crystals and by diminishing the ductility. Further, it had also been ascertained that the influence of phosphorus on the qualities of iron is increased in a very high degree by the simultaneous presence of a large content of carbon, so that the change in its qualities depending on a certain content of phosphorus is much greater in a steel rich in carbon than in an iron poor in carbon. These relations Terrenoire turned to account in the employment of its ferro-manganese. For by its help, it could, as has been already said, without danger of red-shortness, produce a final product so poor in carbon that the injurious influence of phosphorus upon it became much less than it otherwise would have been. Besides, it was possible, without to great an increase in the content of carbon, to obtain in the final product a considerable content of manganese, which had the double advantage that the manganese appeared at the same time to counteract the injurious influence of phosphorus on the iron, and in some degree to increase its hardness. The result of all this is, that while in so simple an object as rails, the quantity of phosphorus that could be permitted in an ingot steel with 0.5 to 0.6 per cent. carbon was scarcely 0.1 per cent., there may now with 0.2 to 0.3 per cent. carbon and 0.5 to 1.00 per cent. manganese be as much as 0.2 to 0.3 per cent. phosphorus. For rolling rails containing so much phosphorus there is required a more powerful rolling train than for purer carbon steel rails, partly because the more phosphoriferous ingot metal requires a greater extension, in consequence of which the ingots must be larger, and partly because ingot metal containing an excess of phosphorus cannot bear to be heated to so high a temperature as the less phosphoriferous. Nevertheless the product is, of course, inferior, both through increased brittleness and diminished hardness; but it appears as if it might be good enough for rails, at least in countries with a mild climate, and great are the advantages which the metallurgist has already been able to draw from this, not only in melting down and re-rolling old iron rails, but also through its being possible to use at Bessemer works a somewhat more phosphoriferous pig than before. In connection herewith I also beg to be allowed to point to the interesting series of experiments on the influence of carbon, phosphorus and manganese, on the physical qualities of iron, shown in the exhibits of Terrenoire. In general these experiments confirm what was before commonly accepted in this way, but there is one thing that forms an exception to this. The tension experiments made in Sweden appeared to show that the percentage of elongation at breaking is diminished with the content of phosphorus, while from the Terrenoire experiments, on the other hand, it appears as if a content of phosphorus of up to 0.3 per cent. had no special influence on the percentage of elongation at breaking. Should this observation come to be confirmed by continued experiments, it would afford the clearest proof of the insufficiency of tension tests alone as a means of judging of the goodness of iron, for the Terrenoire and the Swedish experiments agree in another point, inasmuch as they both show that phosphorus very considerably increases the sensitiveness of iron to blows. Even if tension tests of phosphoriferous iron give excellent results, increased tensile strength and undiminished percentage of elongation, it is nevertheless both in tests of a falling weight and of daily experience a settled matter that an exceedingly small content of phosphorus has an injurious influence on the power of resisting blows even of iron poor in carbon. It is not, therefore to be wondered at if the metallurgist devotes the greatest attention to the important question how phosphorus can be removed from iron. That this may be done to a high degree by suitable puddling at the same time that the quantity of phosphorus remaining in the puddled iron has not so injurious an influence on it as it has upon the more cinder-free refined iron of the Lancashire fire, and in a yet higher degree upon the quite cinder-free ingot iron, are facts which have been long known. This is, perhaps, easily explained by the lamellæ of cinder

counteracting the crystalline texture, with the resulting brittleness which phosphorus produces. Again, that puddling purifies from phosphorus so much more than the other refining processes depends, as is well known, on the circumstance that phosphorus must be removed from iron as a salt of phosphoric acid passing into the cinder, and neither the Bessemer nor Lancashire refining processes admit of this in a degree comparable with puddling. In order that the salt of phosphoric acid may be able to remain unchanged in the cinder, the latter must not be too acid or rich in silica, and its temperature must not be too high, for then the silica drives out the phosphoric acid, which, when set free, is immediately reduced by the carburetted iron with which it comes in contact, and enters into combination with the same. This is the case in the Bessemer process. Again, that Lancashire refining purifies iron from phosphorus in so much smaller a degree than puddling depends, without doubt, on the fact that charcoal in the open hearth is found in contact both with the iron and the cinder; and though the latter is commonly somewhat richer in protoxide of iron than in the case of puddling, and therefore ought to purify still more from phosphorus, this action is neutralized by the pieces of charcoal present, which reduce most of the phosphoric acid contained in the cinder that has passed into it, and thereby returns the phosphorus to the iron.

To how great a degree success has recently been obtained in freeing iron from phosphorus by adding rich iron ore or other materials rich in oxidized iron during puddling. appears very clearly from several French, Belgian, and English exhibits, which, though the ores employed are so phosphoriferous that their pig contains 1 to 1.5 per cent. phosphorus, yet show so beautiful cold worked specimens of their iron, that one not familiar with the facts would have difficulty in believing that the raw materials employed were so rich in phosphorus as in fact they were. All other exhibits of puddled iron are, however, in this respect far surpassed by that of Hopkins, Gilkes, and Co. of Middlesbrough, which show cold-worked samples of such excellence of iron, that one would far more readily believe that they were made from ores nearly free from phosphorus than from those of Cleveland, famous for the quantity of this substance which is found associated with them, and which yield a pig containing 1.5 per cent. This iron is made, as is well known, in rotating puddling furnaces; and it ought to be a pleasure for all who have taken part in the difficulties with which machine puddling has had to contend, to see that untiring perseverance appears at last to have gained its well-deserved reward. It would, however, ill become me to seek to enter further on the question of the superiority of the rotating puddling furnaces over fixed ones, as it is just this honored Association which has spread abroad nearly all the knowledge that is to be found regarding this subject. As, however, at the meetings of this Institute different furnace constructors have sometimes sought to hold out the greater effectiveness in purifying from phosphorus, as specially distinctive each of his own puddling furnace, I cannot omit to give expression to the view that it ought to be a point of superiority, common to all rotating puddling furnaces, that they purify from phosphorus more than fixed ones; for the more the phosphoriferous iron is exposed to the action of the fettling, rich in protoxide of iron, the more phosphorus ought to be removed; and it would be perhaps difficult to bring about in a fixed furnace a contact between these materials so often repeated as is attained by the rotating puddling furnace without manual labor. Iron made in the rotating puddling furnace is also exhibited both by Creusot and by the Compagnie des Forges de Donain et d'Anzin. The latter works has a Crampton's furnace, while Creusot has for more than two years had at work two modified Danks furnaces, with a double plate covering, through which water circulates. Such a furnace is to be seen in the magnificent pavilion of Creusot. The iron made with it is stated to be nearly free from phosphorus, but it is also manufactured from a pig very poor in phosphorus. It is clear from the foregoing that one way of producing ingot metal, even from very phosphoriferous pig, would be first to puddle it in a rotating furnace, and then to fuse the puddled iron thus obtained with pig poor in phosphorus. But, on

the one hand, such puddled iron, notwithstanding the beautiful cold-worked specimens exhibited, is not in general so poor in phosphorus as is desirable for ingot metal of first-rate quality, for Hopkins, Gilkes, and Co.'s iron, according to the analyses given, contains from 0.08 to 0.17 per cent. phosphorus; and, on the other hand, such iron, up to this time at least, has not been made so cheaply that it could be expected to compete in the way that has just been pointed out with Bessemer metal, now so low in price.

The great importance which the question of how ingot metal is to be produced from very phosphoriferous raw materials has, for such a district as that of Cleveland, gave occasion, as is well known, to the very thorough and interesting researches of Mr. I. Lowthian Bell. With the same frankness and love for scientific enlightenment which induced him formerly to lay before this Institute his comprehensive researches regarding the blast furnace, which placed it in an altogether new light, he has also, in several memoirs which have been read with the greatest interest over the whole world, given an account of his attempts to purify pig iron from phosphorus. By these experiments Mr. Bell has, in the most indubitable way, not only confirmed and thrown still further light on what science had formerly more or less thoroughly ascertained in this department, but he has, moreover, succeeded in devising a method of applying on a great scale the scientific results at which he has arrived. He has also communicated so much on this point to this Institute that it would be unnecessary, not to say improper, for me to discuss this subject further, were it not the aim of this paper to endeavor to point out the most interesting objects which are to be found in the Paris Exhibition relating to the manufacture of iron and steel; and what iron metallurgist can well deny that Mr. Bell's exhibit has an interest with which scarcely any other than that of Terrenoire can come into comparison. I ought, therefore, perhaps to be forgiven if, notwithstanding all that Mr. Bell himself has already communicated to this Association regarding his plan of purifying from phosphorus, I, too, beg to say a few words on this subject. For a long time back there has been employed in some districts, as is well known, a preparatory refining process in a separate hearth or furnace, after which the pig which had undergone this process was finally refined to malleable iron in another hearth or furnace. The object of this preparatory refining was partly to diminish the content of silicon in the pig iron, and thereby render it more suitable for the final refining process, and partly to diminish the percentage of phosphorus in the pig iron, and thus obtain a less phosphoriferous final product. Both these objects Mr. Bell has had in view with this process, but he has succeeded far better in attaining them than had been done previously, the reasons of which we shall soon see. In the common running-out fires the pig iron is melted in contact with the fuel, and even if substances rich in oxidized iron are added to it, it is certain that the purification from phosphorus can never in this way be complete; but when we consider the fact already stated, that the Lancashire hearth refining purifies from phosphorus to a very inconsiderable degree, we rather find occasion for surprise that the common running-out process can take away so much phosphorus as it does. The reason, however, lies in the following two differences between hearth-refining and the running-out process:—(1) In the former the phosphorns, which has been taken up by the cinder as a salt of phosphoric acid, comes into simultaneous contact with carbon and more or less decarburetted iron, and it is a fact, which is proved by several circumstances, that iron combines both with phosphorus and several other metalloids with greater attractive force in proportion as it is purer and more refined. In the running-out fire, on the contrary, the pig iron is never decarburetted in any noteworthy degree, and it therefore never acquires so strong a disposition to reduce the phosphorus out of the cinder and again enter into combination with it. In the running-out fire, too, the fused iron in general does not come into simultaneous contact with the cinder and carbon, but a cinder bath is interposed between the fused iron and the carbon, while, on the contrary, the iron during the operations in the refining hearth comes into such simultaneous contact with the cinder and carbon as has as its result the reducing of the

phosphorus and its re-combination with the iron. (2) In the refining hearth the iron is subject during the latter part of the process to a higher temperature than is the case in the running-out fire.

The running-out fire process has exceptionally been carried on in a reverberatory furnace without contact with the fuel, and as the purification from phosphorus which takes place in the puddling furnace is so much more complete than that which is accomplished in the Lancashire refining hearth, we might well have supposed that a reverberatory furnace would be distinguished in the same way in comparison with a common running-out fire. As reverberatory furnaces have been arranged, this, however, has scarcely been the case; and the reason of this is not difficult to find, when we consider that such furnaces have been lined with sand or masses of quartz, which prevent the cinder from being sufficiently basic or rich in oxidized iron; and we ought never to forget the fact already touched upon, that, if any considerable purification from phosphorus is to be brought about, the cinder must always be kept so basic that the silica is well saturated, and so has not too strong a disposition to liberate from the cinder the phosphoric acid, which is then reduced, and enters into combination with the iron as phosphorus. All these defects, inseparable from the old method of refining, Mr. Bell has now succeeded in avoiding by running pig iron rich in phosphorus into a reverberatory furnace, lined with iron ore, or some other substance, rich in oxidized iron, and then, at a temperature not exceeding that which is required to keep the pig fluid, by bringing about, either by the nature of the furnace itself or by stirring, a powerful action of the peroxide of iron on the pig. The result of this has been striking; a ton of molten pig iron, with 1.8 per cent. silicon, 1.4 per cent. phosphorus and 3.5 per cent. carbon, being changed in ten minutes into a product with only 0.05 to 0.1 per cent. phosphorus and 3.3 per cent. carbon. The waste is only about 2.5 per cent. Several different kinds of reverberatory furnaces have been tried for this purpose, but that which for the present is believed to be the most suitable is Pernot's flat furnace on an inclined axle. The refined pig is run out into cakes, which it is then the intention to melt down, along with some rich iron ore poor in phosphorus, in a Siemens regenerative furnace without crucibles, to ingot metal according to the Landore method. Mr. Bell, however, has not for the present any such furnace at his disposal; and the specimens of ingot metal included in his exhibit, accordingly, have not been produced by himself, but have been prepared according to his method from Cleveland pig at Woolwich, where the smelting has proceeded in a furnace of Mr. Price's well-known construction. This has its peculiar interest, as the circumstance that soft steel and iron may be kept fused in Price's furnace further confirms the fact already proved by the low consumption of fuel, that this furnace is in a high degree regenerative. As Mr. Bell's process has only been employed experimentally, it is of course yet too early to give an opinion on its future. The first question with reference to it is, whether it can be got to work so uniformly that the purification from phosphorus will be always equally complete, and the product accordingly quite reliable. This ought best to be attained by the help of a self-acting furnace. The second question is whether this method can be made cheap enough, so that the ingots thereby produced will be able to compete in working expenses with Bessemer ingots. A main factor in judging of these questions is the endurance of the lining of the refining furnace. If it can be got to stand pretty well, the process itself goes on so fast that the refined product must be quite cheap. As, besides, it consists almost exclusively of iron and carbon, its decarburretting with rich ore ought to proceed in a considerably shorter time than is commonly required for the open hearth process, and there thus appears to be a good prospect of producing from a pig, rich in phosphorus, an ingot metal both cheaper and poorer in phosphorus than is possible by machine puddling. The final determining factor will, of course, be the difference in Bessemer pig produced from ores poor in phosphorus, and the Cleveland rich in phosphorus, and Mr. Bell's process ought, therefore, at least, to become a regulator of the excess in price of the sorts of pig which are poor over those which are rich in phosphorus.

As the drawn out ingot metal has recently more and more replaced the wrought iron, steel castings have also more and more encroached upon the territory of iron castings, inasmuch as a great many things, in which more than ordinary strength is required, are now cast in steel instead of iron. For this purpose crucible steel has been used for a long time back, but it has since become more common to employ, not only Siemens-Martin, but also Bessemer steel. The Exhibition is so rich in Siemens-Martin castings, that it would not repay the trouble to enumerate the different exhibitors, but Angleur, in Belgium, ought, perhaps, to be mentioned as exhibiting Bessemer castings of more than common merit. In order that the castings may be considered of first-rate quality, it is, of course, requisite that they be compact, and the greatest difficulty in their production is, as is well-known, just the fulfilment of this main condition. As the blow-holes in steel are caused by the escape of gases which have not reached the upper surface of the casting previous to its cooling, and as, further, this escape of gas arises partly from the gases which the steel has taken up during its formation or melting, and partly from the carbonic oxide which is formed by the action of the oxygen distributed through the steel, or, perhaps, more correctly of oxide of iron upon the carbon of the steel, it is easy to understand that the difficulty of getting steel castings compact is least with crucible melting, greater with the Siemens-Martin, and greatest with the Bessemer process. So long as the castings are made of hard steel, the difficulties in this respect are, however, comparatively easy to get over, but in steel castings a greater ductility is often required than that which hard steel possesses, and it is, therefore, necessary in many cases that the steel be soft, with only 0.5 to 0.6 per cent. of carbon. A very common way of attaining this end is to cast pieces of very hard steel, and afterwards, in the same way as is common in the production of malleable castings, to subject them to heating in a powder of oxides of iron, which diminishes the content of carbon in the steel castings from without inwards. Compact steel castings, with the ductility increased in this way, are also exhibited from several works, as, for instance, by Dalifol in Paris and G. Fischer at Schaffhausen. A method that has been long employed to promote freedom from blow-holes in steel castings is to add a pig iron rich in silicon to the soft steel while it is being melted, for the thus increased content of silicon in the steel counteracts, as is well known, both the taking up of gas during melting and the formation of carbonic oxide during the cooling of the cast steel. The common content of silicon in the products of various works famous for their compact steel castings has, therefore, been about 0.30 per cent. Thanks to its more than ordinary skillful engineers, M. Walton and his successor M. Pourcel, and a management with correct application for the requirements of the times, Terrenoire has now further developed this manufacture by adding at the close of the melting of the steel so-called "fer-manganese-silicium," or a pig iron rich in manganese and silicon. The richest specimen of this which the Exhibition has to show contains 20.5 per cent. of manganese and 10.5 per cent. of silicon. The advantage of this is, that when the oxygen dissolved in the steel or the oxide of iron comes into simultaneous contact with manganese and silicon, both these substances are oxidized, and there is formed a double silicate of protoxide of iron and manganese, more fusible and fluid than the silicate of protoxide of iron, which is formed when only a pig iron is added which is rich in silicon but poor in or free of manganese. Through the greater fusibility and fluidity of the silicate thus formed, there is naturally a diminution of the danger that it will not completely rise to the upper surface of the steel and there separate itself as a layer of slag, but remain in the interior of the casting as a network, and thus diminish its strength. It is clear, however, that it is not necessary for this purpose to use "fer-manganese-silicium," which must be very difficult to manufacture, inasmueh as the obtaining of the greatest possible quantity of manganese in a pig iron demands conditions on the blast furnace burden quite opposite to what is necessary for attaining the greatest content of silicon; for the former requires the minerals not only to be very rich in manganese, but also to be as basic

as possible, while for the production of silicious iron it ought to be as acid as possible. The end in view, viz., the simultaneous addition of manganese and silicon to the steel, ought as easily to be attained by the addition of a fused mixture of ferro-manganese and a very silicious pig, and in such a case the difference is small from the method formerly employed of using ferro-manganese instead of spiegeleisen. The advantage of the Terrenoire process is thus that by means of it we can directly manufacture a softer, and in consequence a more ductile, but still compact product than was previously possible. There are also now produced at Terrenoire only steel castings poor in carbon, for the hardest, or those that are used for armor-piercing projectiles, contain, according to an obliging communication by M. Pourcel, not more than 0.5 to 0.6 per cent. of this metalloid.

It would appear from several publications in technical periodicals descriptive of the Terrenoire process, as if silicon has been found not only to promote the compactness of steel, but also otherwise to improve its qualities. This is, however, by no means the case; but experience at Terrenoire has completely confirmed the old opinion, that the greater the content of silicon in a steel, otherwise of similar quality, the more sensitive it is to blows. The addition of silicon is considered simply as an evil necessary for the sake of the compactness of the steel wares, and great importance is placed on not adding a superfluous quantity of silicon, in order that the content of it in the product may not be greater than is absolutely necessary.

For ingot iron and steel, which are subjected to shingling or rolling, and whose blow-holes, therefore, may be rendered harmless by welding, M. Pourcel will, on no account, employ any addition of silicon. The most common content of silicon in their steel castings is stated to lie between 0.2 and 0.3 per cent., and such a content of silicon is considered pretty harmless. The very considerable percentage of manganese—0.55 to 0.7—which their steel contains doubtless contributes to this, for metallurgists had previously believed that they found that manganese counteracted the injurious influence of silicon on the qualities of iron.

At Terrenoire there has been a higher aim set up by degrees in the production of steel castings, and their very fine exhibit shows that they now even reckon on being able to substitute castings for a number of articles for which malleable iron or steel is used for the present. For besides armor-piercing projectiles, both massive and hollow, and cylinders and other parts of hydraulic presses, there are to be found exhibited not only tubes but also rings for cannon, cranked axles, and other similar unhammered castings. Although all these articles are unhammered, both the surfaces of fracture exhibited and the tension and other tests, the results of which are communicated, show that the physical qualities of the finished products correspond pretty closely with those which distinguish hammered ingot metal with the same chemical composition. On this point, as is well known, various communications have not only been made to this Institute, but others have appeared in various journals, and I, for my part, confess that nothing exerted on me a force so attractive to the Paris Exhibition as just the hope of being able there to find an explanation of the problem, hitherto unexplained so far as I am concerned, by the published communications to which I have referred, viz, How the qualities of ingot steel may be so changed without hammering that they become comparable with those of hammered steel. Nor has this hope been disappointed, for from the Terrenoire exhibit, and the printed description of it, it is clearly evident that this alteration in the qualities of steel is brought about by hardening. A rapid cooling of a large piece of steel heated to a red heat acts upon it in quite the same way as a hammering, for the contraction of the outer layer caused by cooling must bring about a powerful compression of the interior layers. In order, however, that this action be sufficient, it is necessary that the modulus of elasticity of the material be so high that the resistance of the inner layers to the action of the outer do not produce in the latter a set, or permanent extension, whereby the compressing action is diminished. The iron intended for the purpose ought, therefore, not to be too

pure, for the modulus of elasticity of pure iron is, as is well known, very low. But, on the other hand, the content of carbon in the material ought neither to be too great nor the steel too hard, for otherwise it is difficult to modify the hardening that its action be not too powerful when the ductility becomes lessened and the product brittle. In this way it is explained why it appears most advantageous to keep the percentage of carbon between 0.3 and 0.6, the lesser quantity for larger and a greater for smaller pieces, and in general to carry out the hardening in oil. Should the material be rather hard for the intended purpose, the more moderate hardening which is produced by the cooling of the piece in air may be best, or the excessive hardening must be succeeded by a tempering whereby the ductility of the material is increased.

If these explanations of the facts shown by the Terrenoire exhibit be correct, it follows that if the best results are to be obtained, not only the hardening but also the preparation of the steel must be managed with the very greatest care and attention. The melting is carried on at Terrenoire in Siemens furnaces, without crucibles, and Mr. Holley has, in the *Metallurgical Review*, given an interesting description of the way in which the changes of the steel bath succeed each other, and, partly by the help of the appearance of the slag, partly by hammering samples taken out of the bath, the proper moment is determined for adding the compound of iron, manganese, and silicon. For castings, compactness is naturally of greater importance than for ingots, which are afterwards to be drawn out; but even for the latter compactness is far from being a matter of indifference if it can be attained without the sacrifice of any other good quality, for unfortunately the ingot blow-holes are far from being always properly welded together when the ingots are drawn out. It is therefore not to be wondered at, that experiments have been made at many places to prevent the formation of blow-holes by means of powerful hydraulic pressure applied during the cooling and solidification of the cast steel or iron. This plan has been tried at several places, as, among others, at St. Etienne by V. Biétrix et Cie., but it has never been regularly employed except by Sir Joseph Whitworth and Co., Manchester, where, as is well known, this method has been in use for more than ten years. Exceedingly beautiful articles are exhibited by this firm, world-famous for its accurate workmanship, among which may specially be mentioned a hollow cylinder with an interior diameter of 1.98 metre, and a length of 1.5 metre, and a thickness of material of only 4 centimetres, a torpedo guaranteed to resist an interior pressure of air of 105 kilogs. per square centimetre, and a hollow axle 10.26 metres long with an exterior diameter of 45, and an interior of 30 centimetres. All these pieces are made from hollow ingots, which, when under preparation, are exposed to powerful hydraulic pressure, after which the ingot that has been thus treated is further worked by means of a hydraulic compression; but, unfortunately, it is impossible to obtain at the Exhibition any more detailed account of this interesting method of working. Finally, with regard to crucible-melted tool steel, the Exhibition has nothing properly new to offer under this head, if we do not consider chrome steel as such. This, as is well known, is made by adding a pig iron rich in chrome, and such a pig, along with tungsten pig, is found, among others, in the exhibit of Terrenoire. The iron compound richest in chrome, containing up to 65 per cent., is however exhibited by J. Holtzer, Dorian, et Cie.'s steel works at Unieux, near St. Etienne, and it is made by the reduction of chrome ore with charcoal in the crucible. The last-named exhibit also contains the largest quantity of chrome steel. The tension tests to which this steel and the chrome steel from Terrenoire have been submitted have further confirmed the statement previously made in other quarters, that chrome still more than carbon increases, not only the hardness, but also the modulus of elasticity and the tensile strength, while at the same time it does not diminish the ductility so much as carbon. The action of chrome is thus exceedingly advantageous, and much resembles, but is believed to be still more powerful than that of tungsten. Jacob Holtzer's steel, which is richest in chrome, is said to contain 2.5 per cent. The beautiful exhibit of Seebohm and Dickstahl, of Sheffield, also

contains chrome steel, with only 1 per cent. chrome. Wolfram or tungsten steel is shown, not only by the exhibitors of steel just named, but also by several others, among which may be specially mentioned the very beautiful exhibits of crucible steel of, first and foremost, the Innerberger Hauptgewerkschaft, but also of Eibiswald in Styria.

REPORTS OF ENGINEERING SOCIETIES.

AMERICAN SOCIETY OF CIVIL ENGINEERS.—The last issue of the "Transactions" contains the following paper and discussions:

No. 162. The South Pass Jetties: Descriptive and incidental notes and memoranda, by E. L. Corthell.

Discussions on the South Pass Jetties, by C. W. Howell. E. L. Corthell, C. Shaler Smith and J. Foster Flagg.

Addition to Paper No. 160, by James B. Francis.

This number also contains plates, from No. XIV to XX inclusive, illustrating South Pass Jetties, and Plate XXI showing the Mouth of the Magdelena River.

ENGINEERS' CLUB OF PHILADELPHIA.—At the last meeting of the Club, Professor Lewis M. Haupt, President, read a memorial to the State Legislature, praying that an appropriation be made to co-operate with the General Government in the more vigorous prosecution of the Geodetic and Topographical Survey of the State, for the following reasons:

1st. The imperative demand for such work to supply correct maps for the true representation of the geology of the State.

2d. Correct maps are necessary to the proper development of the State.

3d. To reform the system of land surveying now the source of so many uncertainties in consequence of the secular changes in variation of the magnetic meridian; and,

4th. The ultimate economy of accurate surveys.

The memorial closed with a statement of the organization required for such works.

In supporting it, Mr. Ingham, Commissioner for the Second Geological Survey, said that they have found the present maps, boundaries, &c., to be utterly worthless as regards accurate location. In many cases requiring the geology to be forced to fit county lines, and regretted that this State had not already taken steps to remedy this evil. After further discussion action was postponed.

Mr. A. A. Roberts laid before the club the original drawings for structures on the Allegheny Portage Road (1831–6); among others the plan of the first tunnel in America. These he has recently discovered.

A letter from Mr. J. Christie, corresponding member, was read in relation to simplifying formulæ for strains in rolled iron I, T and L beams, giving result of some experiments recently made.

Mr. Henry G. Morris exhibited plans of several boilers, which he had used with good results, and showed comparative merits of each; also plans of sugar-making machinery, with detailed explanations.

Mr. Mucklé presented drawing of Eave's new safety valve, from "Atlas Steel and Iron Works," and showed its advantages. Also read a description of Haddan's Military or Pioneer Railway, recently placed before the Royal Institution, and, when on trial, a section was erected at a speed equivalent to a mile a day for every hundred men employed. This was over uneven ground.

LOUIS C. MADEIRA, JR,
Secretary pro tem.

PREMIUMS FROM THE INSTITUTION OF CIVIL ENGINEERS.—The originality, labor, and ingenuity displayed by the authors of some of the communications submitted to the Society during the session 1877–78 have led the Council to make the following awards:—

For Papers read at the Ordinary Meetings.

1. Telford Medals, and Telford Premiums, to R. W. H. Paget Higgs, LL.D., and J. R. Brittle, for paper on "Some Recent Improvements in Dynamo-Electric Apparatus."

2. A Watt Medal, and a Telford Premium, to H. Davey, for paper on "Direct-acting or Non-rotative Pumping Engines and Pumps."

3. A Telford Medal, and a Telford Premium, to T. Curtis Clarke, for paper on "The Design generally of Iron Bridges of very large Span for Railway Traffic."

4. A Watt Medal, and a Telford Premium, to Bradford Leslie, for paper on "The Hooghly Floating Bridge."

5. A Telford Premium to J. Atkinson Longridge, for paper on "The Evaporative Power of Locomotive Boilers."

6. A Watt Medal, and a Telford Premium, to Alfred Holt, for "Review of the Progress of Steam Shipping during the last Quarter of a Century."

7. The Manby Premium to E. Bazalgette, for paper on "The Victoria, Albert, and Chelsea Embankments of the River Thames."

Other medals were awarded for papers printed in the proceedings without being discussed, and for papers read at the supplemental meetings of students.

IRON AND STEEL NOTES.

STEEL AT THE PARIS EXHIBITION.—The numerous visitors to the Machinery Hall must have observed an exceedingly choice assortment of Messrs. H. Augustus Guy and Company's Specialties, foremost among which figures their well-known invincible tool steel in the ingot, bar and representative tools. We understand that these gentlemen, in the exercise of their undoubted rights declined to admit the jurors into the secrets involved in the materials and manufacture of their monopolies. Consequently their exhibits were not adjudged for awards. Of course, when a firm has devoted years to a valuable improvement, in a commodity like tool steel, and is beginning to

feel the advantages of success, it certainly requires much more than average self-abnegation to disclose the details of their system to the world. They, however, proposed a very ample equivalent, so far as the jurors were concerned, and the public more especially, in their offer to submit sample bars of any size or section for the most crucial tests, in competition with all manufacturers, to make good their claims to the highest honors, and it is to be regretted that this course, which is, after all, the only true criterion, was not adopted. We had an opportunity this week of inspecting at the firm's London office some new specimens, which, we were informed, embody the discovery and successful application of further improvements. Guy's "True" boiler cleaner for removing and preventing incrustations in land and marine boilers was also exhibited, and aroused considerable interest. It has a double value, and meets a very serious difficulty—a problem which a protracted inquiry on the part of our own Government has failed to solve satisfactorily—for it prevents oxidization of the boiler plates, while it also moderates priming, and in this capacity must be of great value.

THE USE OF STEEL FOR STRUCTURAL PURPOSES.—The final report of the committee of the British Association on the use of steel for structural purposes states :—"Having given the subject our best consideration, we recommend that the employment of steel in engineering structures should be authorized by the Board of Trade under the following conditions, namely: (1) That the steel employed should be cast steel or steel made by some process of fusion, subsequently rolled or hammered, and that it should be of a quality possessing considerable toughness and ductility, and that a certificate to the effect that the steel is of this description and quality, should be forwarded to the Board of Trade by the engineer responsible for the structure. (2) That the greatest load which can be brought upon the bridge or structure, added to the weight of the superstructure, should not produce a greater strain in any part than 6½ tons per square inch. In conclusion, we have to remark that in recommending a co-efficient of 6½ tons per square inch for the employment of steel in railway structures generally, we are aware that cases may and probably will arise when it will be proposed to use steel of special make and still greater tenacity, and when a higher co-efficient might be permissible, but we think these cases must be left for consideration when they arise, and that a higher co-efficient may be then allowed in those instances where the reasons given appear to the Board of Trade to justify it." This report has since been acted upon by the Board of Trade in the printed paper issued by them in reference to railway structures. "It will be observed that a co-efficient of 6½ tons per square inch is assigned to steel, that of iron being 5 tons per square inch. This increase of the co-efficient will effect important economy in structures, especially in bridges of large spans, and will also tend generally to increase the employment of steel for railway and shipbuilding purposes. The labors of your committee having ended in such a satisfactory manner there is no necessity to re-appoint them." The report is signed by Mr. E. H. Carbutt, Mayor of Leeds, as Secretary.

THE MECHANICAL AND OTHER PROPERTIES OF IRON AND MILD STEEL.—Numerous experiments have been conducted by several eminent engineers to prove the tensile strength of iron and steel, both in the shape of bars and plates. Unfortunately, however, many of the tests have been carried out with rude testing machines, rendering it difficult to obtain a true result of the endurance and strength of the metal under investigation. Some experiments were conducted with a view to determine the strength of steels with fixed proportions of carbon only, by Mr. Vickers, of Sheffield, and recorded by him in a paper read on the subject before the Mechanical Engineers of England, August 1st, 1861; but as these tests were more especially resorted to to ascertain the strength of crucible steels, mostly used for tool-cutting purposes, they were of but little value to the constructive or mechanical engineer. Mr. Adamson, having used practically a comparatively mild class of steels or ingot irons for the last twenty-one years, at times found, from cold mechanical bending tests, some irregularities in the working of the metals. This indicated to him the necessity of more careful investigation, both as to their composition and the temperature at which they could be manipulated in the workshop and practically applied; and in the present paper his object was to put before the members a record of the endurance of iron and steel when subject to concussive force such as can be produced by gun-cotton, gunpowder, or other explosive materials. The experiments carried out were instituted with a view to ascertain what would be the effect on a steam-boiler working under pressure by the side of an exploding boiler, or the effect on a ship by a collision with another, and whether wrought iron or steel possessed the greater power to resist such accidentally produced force. Uniformly the various trials made by the writer in June, 1876, were favorable to mild steel. Drift and tensile tests pointed emphatically in the same direction. The value of steel and iron for structural purposes was also tested, and contrasted with that of iron, the result being to show that steel with about one-half per cent. of carbon, 1 per cent. of manganese, with a low measure of silicon, sulphur and phosphorus, can be depended upon to carry double the load of the best wrought-iron plates that can be produced, and with as good results as regards elongation. After many trials and many failures in attempting to weld steel boiler plates, the writer found it necessary to ascertain in all cases the composition of the metal before putting any labor upon it. From a large experience it is now found desirable that the carbon should not exceed one-eighth per cent., while the sulphur and phosphorus should, if possible, be kept as low as .04 silicon being admissible to the extent of one-tenth per cent. The writer then passed on to describe a variety of tests of the malleability of iron and steel, their powers of endurance under color-

heat, &c., and followed with the observation that from the experiments he had explained, it would be apparent that the users of metals must make some natural selection, as it were, to secure the highest and best results for any special purpose. It would also be clear that no wrought iron could resist concussive force equal to mild steel, and as a much higher range of ductility and carrying power was attained, he had no doubt constructive engineers would feel themselves constrained to use it much more extensively in all cases where strength and lightness were required. Should it ultimately be proved that sea-water would destroy steel quicker than wrought iron, the use of wrought iron for the skins of ships might be continued; but, with present knowledge, nothing, in his opinion, existed to prevent the whole framework of every steamer and sailing vessel being constructed of Bessemer or Martin-Siemens steel, as at least one-third the weight might be saved at the same time that greater security was ensured. In the diluted sulphuric-acid bath the evidences were quite clear in favor of mild steel and the purest iron to resist corrosion, but before as much could be said as to the influence of sea or salt water a more extended and careful series of experiments would be required. The same might be said of the selection of metals for the construction of artillery; and the writer had no doubt that, by a still more careful manufacture, to keep down the carbon and injurious alloying substances common to wrought iron, most enduring armor plates might be manufactured by the Pneumatic or Martin-Siemens process. Further, there could be no doubt that the medium hard class of steels, possessing double the strength of the best wrought iron that can be made, ought, without exception, to be used for building bridges and numerous other like structures.

RAILWAY NOTES.

A NARROW-GAUGE railroad has been proposed in Guatemala, and agents are now in San Francisco for the purpose of interesting capitalists in the scheme. It is understood that a section of thirty miles, to penetrate the coffee region, will be first made, and, if successful, the road will be extended to the capital of the State. Should the necessary capital be secured in San Francisco, it is claimed that the trade of Guatemala will be attracted to that city. If the necessary aid cannot be secured there, an appeal will be made to the capitalists of the East or of Europe.

OF all the sources of railway disasters, shunting operations are perhaps the most prolific; but this truth has either failed of appreciation by railway directors, or satisfactory means of removing the danger have not appeared. Among other inventors who have attempted this, however, Mr. Barrow, of Rock Ferry, Liverpool, has recently finished an apparatus for the protection of sidings during shunting operations. The signal consists of a revolving signal and lamp fixed in the six-footway, 2 ft. high, some 500 and 800 yards from the point, and worked by the points-man in the signal-box. The lever or wheel which works the light also manipulates a couple of fog signals. When the light is turned against a coming train the fog signals are placed on the line by mechanical means, so that should the driver miss seeing the light, the fog signals warn him in time to avert disaster.

GREAT activity is just now being shown in the Austro-Hungarian Empire in the prosecution of all kinds of public works, and especially of those in any way relating to the extension of the railway system of the country. Amongst others, there is a talk of the construction of an iron bridge over the Drave at Eszeg, to replace the present ferry, at a cost of 800,000 fls., which would be carried out partly by the Government and partly by the Alfoeld and Fiume Railway Company, which is domiciled at Pesth. It is also proposed to replace by iron bridges all the wooden bridges on the line worked by the Alfoeld and Fiume Railway Company, and the Kaschau and Oderberg Railway Company, &c., and the construction of the proposed lines of railway on the military borders of Croatia and Slavonia is to be offered for public auction—in fact, according to *Herapath*, one line has already been adjudged.

THE supplement to the last *Gazette of India* contains some interesting statistics of the number of servants of all races employed on the different railway lines in India. The grand total for 7,278 miles of line is 132,040, or between eight and nine individuals per mile. Of these 132,040 persons, 125,040 are natives, 3,319 are Eurasians—children of Europeans but born in Asia—and 3,607 are Europeans. Again, of the total number 8,837, of whom 8,257 are natives, 271 Eurasians, and 309 Europeans are employed in the department of general administration; 31,616, of whom 29 339 are natives, 1,233 Eurasians, and 1,044 Europeans in the traffic and telegraph departments; 52,259, of whom 51,631 are natives, 248 Eurasians, and 380 Europeans in the engineer's department; and 39,328, of whom 35,787 are natives, 1,567 Eurasians, and 1,874 Europeans, in the locomotive and carriage departments. The first thing that strikes us about these figures is the enormously large proportion of natives, not only in the total, but in every individual branch of the work. In fact, it may almost be said that the working of the railways is practically in the hands of the natives of the country—in some cases, but not in all, under European supervision. The insignificant number of Eurasians employed is hardly less striking. In one department alone—traffic and telegraph—does it exceed that of the Europeans. Turning again to the statistics of casualties, we find that among an average number of 3,513 Europeans employed in the year ending 30th September, 1877, there were only eighty-three deaths, while among an average number of 3,319 Eurasians employed there were only thirty-nine deaths, giving about half as high a death rate for Eurasians as for Europeans. The dismissals were 289 and 256 respectively, showing no great disparity between the two classes.

RAILWAY ACCIDENTS.—The *Annales des Ponts-et-Chaussées* has just published some interesting statistics on the above named subject. In the old days of diligences, or stage-coaches, one passenger was killed out of about 335,000, and one wounded out of 30,000; while out of 1,784,404,687 persons carried by the French railways from September 7th, 1835, to December 31st, 1875, only one was killed out of 5,178,490, and one injured out of 580,450. If the accidents are divided into two groups, from September, 1835, to December, 1855, and from January, 1856, to December, 1875, we find that in the first period one traveler was killed out of 1,955,555, and one injured in 496,555. In the second, the proportions were one killed out of 6,171,117 passengers, and one injured in 590,185. As is seen, the number had considerably decreased in the second period. Of late years, the proportion has still further diminished, and the results for such countries as France, England, and Belgium are particularly striking. In France, during the years 1872, 1873, 1874, and 1875, one passenger was killed out of 45,258,270, and one hurt in 1,024,360. In England, from 1872, to 1875, one was killed out of 12,000,000 persons carried, and one wounded in 366,000. In Belgium, from 1872, to 1876, one was killed out of 20,000,000 passengers, and one injured in 3,500,000. To sum up, a person had, in France, in the time of the diligences, a chance of being killed in making 300,000 journeys, and of being hurt once in making 30,000. On the railways, from 1872 to 1875, the chances were reduced to one death in 45,000,000 journeys, and one injury in 1,000,000. Thus, a person continually traveling by rail, at a speed of 50 kilometers ($\frac{5}{8}$ of a mile each) an hour, would have had, during the three periods above indicated, the following chances of being killed: From 1835, to 1855, once in 321 years; from 1855 to 1875, once in 1,014 years; and from 1872 to 1875, once in 7,450 years.

QUEENSLAND RAILWAYS.—The Queensland Minister of Works has intimated that the Queensland Government has no intention of undertaking the construction of proposed branch lines from Ipswich to Fassifern, in one direction, and to Mount Esk in the other, unless the residents in the districts to be benefited by their construction contribute towards the cost, which, we suppose, means that a system of rating railway districts is in contemplation. The proposed branch line from Oxley to Beenleigh, is one with regard to which considerable pressure is likely to be brought to bear upon the Government; but a contention has arisen in favor of a diversion of the route so as to serve the settlers of the Upper Logan and the Albert. The Colonial Sugar Refining Company of Sydney are forming an establishment on the Tweed river, immediately south of the Queensland Border, for the production of sugar on an extensive scale. They have purchased 10,000 acres of land, intending in the first instance to grow their own cane, in the expectation, however, that as soon as machinery has been erected for crushing and refining purposes, farmers will settle in the neighborhood, and enable the company to adopt the plan which they have found eminently successful on the Clarence river, where they last year exported about 7,000 tons of sugar, none of which was from cane of their own growing. But the Tweed is difficult of navigation, and the company have asked the Queensland Government to construct a short line of railway or tramway from the Border to Nerang Creek, Queensland, with the view of making that the port for the produce of the Tweed district. The company further ask whether, in the event of the government declining to undertake the work, they will permit the company to make the line, and, if so, upon what terms. The length of line will not exceed 15 miles.

ENGINEERING STRUCTURES.

THE work of tunneling the St. Gothard Railway is being pushed on with considerable rapidity. A telegram from Geneva states that on the Goeschenen side alone 1,000 men are employed inside the tunnel and 400 outside. Three hundred wagon loads of earth are excavated every day, and in the daily blastings 600 lbs. of dynamite are used. Equal energy is being shown on the Italian side.

THE ALTENBURG TUNNEL.—H. Von Oer gives a full and detailed account of the system of supports adopted at the Altenburg tunnel, where iron was made use of in place of the usual timbering. The system, as there carried out, is due to Herr F. Rziha, an Austrian engineer, and is a modification of that in use in the Saxon mines for the timbering of drifts. The author claims that it possesses all the advantages of the English system, as designed by Brunel, without its defects. It consists essentially in the adoption of the arched form, embracing the whole section of the tunnel, the structure being built up of short segments, varying in length from 1 metre to 1½ metre (3.28 to 4.9 feet), composed of angle iron, and joined together by the flanges. A characteristic feature of the system lies in the application of the common forms of angle iron, by which means economy in the cost of the materials is secured. Herr Rziha makes large use indeed of old rails; and the paper gives a drawing showing the arrangement of these materials. The construction is a kind of double arch, the outer ring of which supports the earth, and itself rests upon the inner ring, which is designed to serve as the centering upon which the masonry is to be built in. As the work of excavation advances, the outer ring supporting the rock is removed in small portions at a time, and the bricking is built up upon the lower ring. The distance between the two rings being made to correspond to the required thickness of the arch. The several parts of the structure are simple in form, light, and easily put together. The erection is carried on, as the excavation progresses, in a manner similar to that followed in the ordinary method of timbering. The system is said to be a very efficient one, and a tabular statement of quantities and cost shows it to be also remarkably cheap. In one case, the econ-

omy resulting from the adoption of iron instead of wood, amounted to as much as 84 marks per lineal yard, the estimates being 328 and 412 marks respectively. The article is illustrated by general and detail drawings, which show clearly the design and mode of construction adopted at the Altenburg tunnel. The same system, the author remarks, is at the present time being made use of in the Remsfeld tunnel, 900 metres (984 yards) in length, on the line of railway from Berlin to Coblenz.—*Abstracts of Institution of Civil Engineers.*

ORDNANCE AND NAVAL.

STEAM STEERING GEAR.—One of our correspondents in Lancashire writes:—"A new steam steering gear, patented by Mr. Harrison, was on Wednesday exhibited for the first time at the works of Messrs. Hodgson and Stead, engineers, Salford. By this invention Mr. Harrison claims to secure to the helmsman a perfect control over the steering engines, and also to do away with the noise which is so objectionable in the apparatus now in use on some of the steamships. The first object is attained by means of a rotary disc valve operated upon by the steering wheel, which cuts off the steam automatically and controls the action of the piston rod to within ¼ inch, the engine, in fact, responding instantaneously to every motion of the steering wheel, whilst the noise is obviated by the substitution in the working gear of a worm in the place of the usual wheels and pinions. It is also claimed that the engine will exert the power of twelve men on the rudder, which will be kept steady however rough the action of the sea may be upon it. The working of the apparatus appeared to give satisfaction to a number of gentlemen who inspected it, but I understand it is shortly to undergo a practical test on board ship at Liverpool.

RUSSIAN FAST SAILING STEAMERS.—The Moscow Cruiser Committee has definitely decided that, if possible, no more war steamers for the volunteer fleet are to be purchased out of Russia The question was raised at a recent sitting of the executive branch of the committee at St. Petersburg, under the presidency of Mr. Pobairdonositz, and after the plans and tenders received from shipbuilders and shipowners in every part of the world had been carefully examined, the members unanimously decided that an attempt should be made to encourage the shipbuilding trade of Russia by giving all future orders to native firms. Thereupon Mr. Baird, of Baird's Engineering Works, and Mr. Kazi, the managing director of the Baltic Iron Works, who were both present, undertook to furnish plans of fast-sailing steamers. A temporary contract was drawn up, the main features of which were that the cruisers designed should be corvette shaped, with a spread of 21,000 square feet of canvas, stowage for sufficient coal to enable the vessel to steam sixty days at full speed, and artillery arrangements for the reception of two seven-inch guns and four four-inch mortars. It was understood that in the event of the designs being satisfactory Messrs. Baird and the Baltic Ironworks would each receive an order for at least one cruiser, and that if the donations continued to come in as largely as at present further orders would be given.

THE HECLA; TORPEDO DEPOT SHIP.—The Hecla, screw torpedo depot ship, which arrived at Portsmouth last week from Belfast, and which is expected to be commissioned today by Captain Morgan Singer, lately in command of the Vesuvius and the Glatton, is altogether a novelty, no other ship of the kind being in existence, and is another concession to the necessities of the new mode of conducting actions at sea. She is to be fitted to carry fast torpedo launches and to follow in the wake of a fleet as a depot, ready to despatch her flotilla of small craft for their protection when necessary. She is constructed of iron, and measures 390 ft. in length, and is fitted to carry six 64-pounder muzzle-loading rifled guns, four on the broadside and the rest forward and aft. She is also intended to be armed with torpedoes of the Whitehead kind, and is pierced with a broadside port on each side for ejecting them. The after part below is furnished with lathes and drilling and shaping machines, and will be converted into a floating torpedo workshop. She is divided into a number of various watertight compartments, not connected, as is the usual mode, with water-tight doors, entrance being gained from the upper and main decks. The element of danger resulting from leaving the connections open in certain eventualities is thus obviated, though it is calculated that the filling of one or two of the compartments with water would not materially affect the behavior of the ship. She is to carry six second class torpedo boats, of which, however, only two have as yet been supplied. Four of these boats will be amidships, the chocks on which they rest running on a tramway. She will also carry a 42 ft. steam launch and a 37 ft steam pinnace. The Hecla will be provided with booms and nets to protect her from an enemy's torpedoes, the booms, when not in use, lying fore and aft against the side of the ship. The captain's cabin and the wardroom are amidships, the wardroom being what, when the ship was built for the merchant service, was intended as a saloon for passengers. She will have a complement of 170 officers and men, and when completed at Portsmouth will be taken to sea for a short period on special service for the purpose of testing her manœuvering and sea qualities.—*London Times.*

STEERING OF SCREW STEAMERS.—The following is the report of the Committee of the British Association, consisting of James R. Napier, F.R.S., Sir W. Thomson, F.R.S., W. R. Froude, F.R.S., J. T. Bottomley, and Osborne Reynolds, F.R.S., Sec., appointed to investigate the effect of propellers on the steering of vessels.

It appears, both from the experiments made by the committee and from other evidence, that the distance required by a screw steamer to bring herself to rest from full speed by the reversal of her screw is independent, or nearly so, of the power of the engines; but depends

on the size and build of the ship, and generally lies between four and six times the ship's length. It is to be borne in mind that it is to the behavior of the ship during this interval that the following remarks apply :—

The main point the committee have had in view has been to ascertain how far the reversing of the screw, in order to stop a ship, did or did not interfere with the action of the rudder during the interval of stopping, and it is as regards this point that the most important light has been thrown on the question of handling ships. It is found an invariable rule that, during the interval in which a ship is stopping herself by the reversal of her screw, the rudder produces none of its usual effects to turn the ship, but that, under these circumstances, the effect of the rudder, such as it is, is to turn the ship in the opposite direction from that in which she would turn if the screw were going ahead. The magnitude of this reverse effect of the rudder is always feeble, and is different for different ships, and even for the same ship under different conditions of loading.

It also appears from the trials that owing to the feeble influence of the rudder over the ship during the interval in which she is stopping, she is at the mercy of any other influences that may act upon her. Thus the wind which always exerts an influence to turn the stem (or forward end) of the ship into the wind, but which influence is usually well under the control of the rudder, may when the screw is reversed become paramount and cause the ship to turn in a direction the very opposite of that which is desired. Also, the reversed screw will exercise an influence, which increases as the ship's way is diminished, to turn the ship to starboard or port according as it is right or left handed; this being particularly the case when the ships are in light draught.

These several influences, the reversed effect of the rudder, the effect of the wind, and the action of the screw, will determine the course the ship takes during the interval of stopping. They may balance, in which case the ship will go straight on, or any one of three may predominate, and determine the course of the ship.

The utmost effect of these influences when they all act in conjunction, as when the screw is right handed, the helm starboarded, and the wind on the starboard side, is small as compared with the influence of the rudder as it acts when the ship is steaming ahead. In no instance has a ship tried by the committee been able to turn with the screw reversed on a circle of less than double the radius of that on which she would turn when steaming, ahead. So that even if those in charge could govern the direction in which the ship will turn while stopping, she turns but slowly, whereas, in point of fact, those in charge have little or no control over this direction, and, unless they are exceptionally well acquainted with their ship, they will be unable even to predict the direction.

It is easy to see, therefore, that if on approaching danger the screw be reversed, all idea of turning the ship out of the way of danger must be abandoned. She may turn a little, and those in charge may know in what direction she will turn, or may even, by using the rudder in an adverse manner, be able to influence this direction, but the amount of turning must be small and the direction very uncertain. The question, therefore, as to the advisability of reversing the screw is simply a question as to whether the danger may be better avoided by stopping or by turning. A ship cannot do both with any certainty.

Which of these two courses is the better to follow must depend on the particular circumstances of each particular case; but the following considerations would appear to show that when the helm is under sufficient command there can seldom be any doubt.

A screw steamship when at full speed requires five lengths, more or less, in which to stop herself ; whereas, by using her rudder, and steaming on at full speed ahead, she should be able to turn herself through a quadrant without having advanced five lengths in her original direction. That is to say, a ship can turn a circle of not greater radius than four lengths, more or less (see *Hankow*, *Valetta*, *Barge*) so that if running at full speed directly on to a straight coast, she should be able to save herself by steaming on ahead and using her rudder after she is too near to save herself by stopping; and any obliquity in the direction of approach or any limit to the breadth of the object ahead is all to the advantage of turning, but not at all to the advantage of stopping.

There is one consideration, however, with regard to the question of stopping or turning, which must, according to the present custom, often have weight, although there can be but one opinion as to the viciousness of this custom. This consideration is the utter inability of the officers in charge to make any rapid use of their rudder so long as their engines are kept on ahead. It is no uncommon thing for the largest ships to be steered by as few as two men. And the mere fact of the wheel being so arranged that two men have command of the rudder, renders so many turns of the wheel necessary to bring the rudder over that even where ready help is at hand it takes a long time to turn the wheel round and round so as to put a large angle on the rudder.

The result is, that it is often one or two minutes after the order is heard before there is any large angle on the rudder, and of course, under these circumstances, it is absurd to talk of making use of the turning qualities of a ship in case of emergency. The power available to turn the rudder should be proportional to the tonnage of the vessel, and there is no mechanical reason why the rudder of the largest vessel should not be brought hard over in less than 15 seconds from the time the order is given. Had those in charge of steamships efficient control over their rudders, it is probable that much less would be heard of the reversing of the engines in cases of imminent danger.

BOOK NOTICES.

PRANG'S STANDARD ALPHABETS. Boston: L. Prang & Co. Price $5.00. For sale by D. Van Nostrand.

This collection of ornamental alphabets for the use of decorators, designers and draughtsmen, is in excellent style.

We are glad to see that in the more florid ornamenting, the letters are yet plainly distinguishable, which was not the case in the letter books of former years.

In addition to the alphabets, there are some examples of topographical mapping in colors, and the Coats of Arms of the States also in colors. Altogether, it is an elegant and useful volume.

A Practical Treatise on Casting and Founding. By N. E. Spretson. London: E. & F. N. Spon. Price $7.00. For sale by D. Van Nostrand.

This book is for the artisan only. It affords a complete description of all the details of casting and founding, iron, steel, brass and bronze.

The illustrations alone cover eighty-four full page plates of royal octavo size.

The work is divided into thirty chapters, but, without enumerating these, the following list of subjects may be mentioned, as embracing the matter of the book in their order:

Pig Iron; Furnaces and their Accessories; Moulding and Casting; Foundries and their Equipments; Steel, Brass, Bronze and Bell Founding; Tables and Notes.

There are 400 pages of text, besides the the plates mentioned above.

Van Nostrand's Science Series, No. 39.

A Hand Book of the Electro Magnetic Telegraph. By A. E. Loring. New York: D. Van Nostrand. Price, boards, 50 cts.; cloth, 75 cts.; half mor., $1.00.

Instruction books for students in telegraphy have heretofore been encumbered with material which was of little or no aid to the beginner.

A small hand book of first principles has been needed to prepare the learner for the preliminary work as well as for the understanding of the complete treatises upon this comparatively new branch of industry.

For a student may be well up in electricity and magnetism of the schools and colleges, and entirely unlearned, not only in the application of the principles of these sciences, but of the technical language of the telegraph room.

Mr. Loring is a practical telegrapher, and has presented in the most concise form the leading facts and formulas which are in constant requisition in telegraphing.

Without being severely technical, or even rigorously scientific, he enables the student to make a good *reconnaissance* of this field of labor, and affords him such hints as will enable him to fill in his details of information from the more complete sources.

The work is divided into parts as follows:

Part 1, Electricity and Magnetism; Part 2, the Morse Telegraph; Part 3, Batteries; Part 4, Practical Telegraphy; Part 5, Construction of Sines.

Appendix containing suggestions and exercises for learners.

The illustrations are good, and are distributed throughout the text.

Coal and Iron in all Countries of the World. By J. Pechar. London: Simpkin, Marshall & Co. Price $2.00. For sale by D. Van Nostrand.

This is largely statistical as the title implies. It is compiled from the latest sources, and is one of the reports made up from materials furnished by the Paris Exposition.

The report deals with the character of the coal and iron deposits, methods of working, and amount of home consumption and export.

The introduction under the head of General Remarks, discusses the causes of the great depression in trade, and adds more valuable statistical information regarding the railway systems of the world.

A History of the Growth of the Steam Engine. By Robert H. Thurston, A.M., C.E. New York: D. Appleton & Co. Price $2.50. For sale by D. Van Nostrand.

This is the latest addition to the "International Scientific Series" of these enterprising publishers, and judging by the naturally widespread interest in the subject, we may expect that it will be regarded as the most important of the series by a large plurality of scientific readers.

The preparation of such a history could not have been assigned to better hands. Taste, early education and professional training have all tended to prepare the talented author for this work, and his experience furthermore as an instructor of young men has specially fitted him to relate the story of the growth of this great agent of civilization, so that the merest tyro can enjoy it, and the scientist regard it as valuable.

Not a small portion of the labor and expense of the work, either to author or publisher, is represented by the illustrations, which are very numerous and exceedingly good.

The book is sure of a multitude of readers.

The Analytical Theory of Heat. By Joseph Fourier. Translated by Alexander Freeman, M.A. Cambridge: University Press. Price $7.00. For sale by D. Van Nostrand.

One of those works involving the higher analyses to an extent that is specially attractive to the mathematician.

When great laws of phyics and their resultant phenomena are expressed by aid of triple integrals, the mathematician first feels an interest in them, and then only proposes to aid in the work of developing.

The department of Heat has long since become a favorite field for the analyst, and the work before us is the most complete evidence of it.

The topics treated by chapters are:

1. Introductory; Equation of the Movement of Heat; Propagation of Heat in an Infinite Rectangular Solid; Linear and Varied Movement of Heat in a Ring; Propagation of Heat in a Solid Sphere; Movement of Heat in a Solid Cylinder; Propagation of Heat in a Rectangular Prism; Movement of Heat in a Solid Cube; The Diffusion of Heat.

It is a well printed volume of 466 pages, royal octavo.

GEOGRAPHICAL SURVEYING. By FRANK DE YEAUX CARPENTER. New York: D. Van Nostrand. Price 50 cts.

This little treatise, written originally, as it appears, for the purpose of presenting to the Geological Commission of Brazil a general sketch of the plan proposed for mapping the immense territory of that Empire, in connection with the Geological Survey organized by the late Prof. Hartt, appears in Van Nostrand's excellent Science Series, and forms a useful contribution to the popular science literature of our country. Its author, formerly connected with the geographical surveys of the Engineer Department under Lieut. Wheeler, proposes the name Geographical rather than Topographical Surveying, to distinguish the kind of work necessary for covering a large extent of comparatively unexplored country (when thousands of square miles must be mapped in a season) from the slow and detailed surveying which indicates every man's farm and house, as carried on by the Government surveys of Europe. While the former should be based on determinations of primary points no less accurate than the latter, the intermediate details are to be sketched in by methods of approximation, which will present with sufficient accuracy the general physical features of the region surveyed, and the method may therefore be called *geo*-graphical rather than *topo*-graphical, as describing the surface of the globe, rather than of limited regions or places. This has been the system pursued by our various Government geological surveys in the Rocky Mountain region; and the author mentions the work of Hayden's, Powell's and Wheeler's surveys, from whose experience he has drawn his material, but neglects to give credit to the forerunner and, in one sense, the originator of all these, that of the 40th Parallel under Mr. Clarence King. As he avoids all formulas, and presents his subject with clearness and precision, the work will be found pleasant reading for all interested in geography.—*The Nation.*

THE ELEMENTS OF GRAPHICAL STATICS AND THEIR APPLICATIONS TO FRAMED STRUCTURES, WITH NUMEROUS PRACTICAL EXAMPLES OF CRANES, BRIDGE, ROOF AND SUSPENSION TRUSSES, ETC. By A. JAY DUBOIS, C.E., Ph.D. New York. 1875. John Wiley & Son.

In the course of a review of DuBois' "Graphical Statics," published in the *Zeitschrift des Ver. Deutsch Ing*, the writer says:

"This surprisingly long title is followed by a preface of ten closely-printed pages, which contains notices valuable to the student while using the book. The table of contents, of twelve pages of fine print, is preceded by a four-page note, 'Elements of Graphic Statics,' intended especially for student and teacher. Then follows, under the title 'Introduction,' an excellent and exact translation (!), including references, of the capital work of our German colleague, Dr. J. Weyrauch, '*Ueber die Graphische Statik*,' *Leipsig: Verlag von Teubner*. The title of the first chapter, 'Historical and Critical,' is accompanied by an asterisk with the reference 'Weyrauch, U. S. W.'; and in his preface DuBois says: 'For the historical and critical introduction we are indebted, a few alterations excepted, to the pen of Weyrauch. It will be useful, &c., &c.' As regards the 'few alterations' of DuBois, we have not been able to discover them, except in the omission of several scientific references of Weyrauch. The American reader is led to infer from DuBois' method of reference that only one page of his 'Introduction' is taken from Weyrauch; when, in fact, as I find after a thorough examination, there are twenty-seven pages of close translation.*

"What particular use was made of Culmann, Mohr, Ritter, Winkler and Reuleaux, and how much Cremona, Favaro and others were studied, after the entire literature had been collated by Weyrauch's diligence for the benefit of the translator, we shall not determine: but to DuBois belongs the credit of industry in collecting, and of the introduction of practical examples."

The reviewer then speaks favorably of the work as a record of the progress of research in this department in Germany, Italy, France and England. Concerning the plates, he says: "Entire plates show a lack of the care in delineation which is required in a work like this."

A HANDBOOK OF PATENT LAW OF ALL COUNTRIES. By WILLIAM P. THOMPSON, C.E. London: Stevens & Sons; New York: Van Nostrand, 1878.

The author of this little book is the head of a patent agency in Liverpool, and therefore writes with the advantage of practical experience. The book has no pretension to be regarded as a complete treatise on patent law; it is rather a guide to patentees, and in many respects an *aide memoire* to practitioners. The first part is naturally devoted to a summary of the English law, in which the progressive steps, with their cost, towards the completed patent, are clearly explained. The suggestions and observations of the author, as for instance those under the head of preliminary "Searches," are generally practical, but there are a few slips which should be corrected in a subsequent edition. At the outset his statement of the principle of our patent law as "a simple contract between the Crown, on behalf of the nation at large, and the inventor," is not legally correct. This view of the relationship of Crown and inventor was judicially repudiated in the celebrated action of *Feathers* vs. *the Queen*, in which Cockburn, C.J., speaking for the Court, explained the grant of a patent to be a mere act of the prerogative, coupled with a condition, namely, full publication by the patentee. Again, Mr. Thompson says of joint patentees that "each can grant licenses independently of the other," omitting to point out that it is by no means clear that the royalties will not belong to both. The well-known case of *Mathers* vs. *Green* decided that a joint patentee could work the whole invention for his own benefit without accounting to his fel-

* In the same way Reye, *Geometrie der Lage*, and Bauschinger, *Graphische Statik*, are employed; of course with references.

low-patentee ; but that decision expressly left open the case of profits to be derived from the grant of licenses. Under the head of "Infringements," Mr. Thompson writes thus: "Patent trials are proverbially expensive in England, the law and procedure being apparently framed with the special object rather of putting fees into the lawyers' pockets than of doing justice promptly and cheaply. As the cases have to be fought out by lawyers, almost invariably utterly ignorant of the technicalities of the case, and before judges, learned only in the law, the probability of obtaining justice, even with a long purse, is not extravagantly great. Often, too, when the case comes to a hearing, and nearly all the expenses of the lawsuit have been incurred, the court, conscious of its poor qualification for deciding scientific and technical matters, persuades the parties to put the matter to arbitration." We are surprised to find any one with any pretence to experience writing in this strain. Surely Mr. Thompson must know that the actual cost of preparing pleadings and bringing the action to issue is trifling to a degree compared with the costs of witnesses and the collection of evidence—costs unavoidable so long as novelty and utility are essential to a patent. He might as justly say that the law and precedure were framed with the object of benefiting professional expert witnesses—one at least of whom, by the way, well known for his ability, is actually a patent agent. That our courts are incapable of dealing with technical cases is amply disproved by the way in which the celebrated Plimpton skate was handled by Bench and Bar in the many actions in which it was involved. As for arbitration as a solution of an infringement queston, we can only say that if a party or his adviser is sufficiently foolish to consent to such a course—and presumably Mr. Thompson has met with a case, we have not—and so preclude himself from judicial assistance, he has only himself to blame. No court in this country declines, or can decline, to try such an action, if properly presented for its decision. Moreover, it is not the fact, as stated further on, that the court rarely makes use of its power to grant an interim or "preliminary" injunction until the trial is decided. This is true in the case of new and untried patents, but where the validity of a patent has been established in another action, such an injunction is almost of course. A great part of the book is occupied by a very useful analysis of foreign laws. So far as we have tested this digest it is clear and correct. We would, however, suggest a few additions. In every case the date, or other reference, to the particular law should be given, and the Government taxes should be inserted. This latter is not always done in the book before us, though it is true Mr. Thompson gives invariably the approximate cost of obtaining the patent—including therein the agent's fees of course. Moreover, it should be stated with more precision whether preliminary examination is or is not required. Such information for instance, is wanting here under the head of "Belgium." The work concludes with "Hints for Inventors," "How to Sell a Patent," and some well-merited strictures on a certain class of "Patent Agents." The defects we have indicated do not seriously affect the utility of the book. It contains a good deal of information in a small space, and will be found useful by a large section of our readers.—*The Engineer.*

MISCELLANEOUS.

HEIGHT OF JETS.—J. F. Flagg, C. E., gives, in a communication to *Engineering News*, a new formula for jets of water.

It is

$$h = H - .00127\ H^2$$

H being the head of water, and h the height of the jet.

GLASS-CLOTH.—*Gastach*, or glass-cloth, is a name given by Dr. Hirzel, of Leipsic, to a gas and water-tight stuff, which he has recently patented. This is produced by placing a large smooth piece of so-called gutta-percha paper between two pieces of some not too coarse and dense material—*e.g.*, shirting (undressed),—and then passing the arrangement between heated rollers. The outer pieces of the shirting combine in the most intimate way with the enclosed gutta percha to form a material which is impenetrable by gas and water. It may be made still denser and more resistant by being coated on both sides with, *e.g.*, copal lac. The material is said to be well adapted to form gas-tight membranes for regulators of pressure of compressed gas-bags, or sacks for dry gas-meters, as also dry gas-reservoirs.

A NEW METHOD OF DETERMINING THE HEAT VALUE OF FUEL.—With regard to the important question of the heat value of fuel, it has been proved that conclusions from the results of elementary analysis are very uncertain, and, also, that little reliance can be placed on direct evaporization experiments. In a recent paper in *Die Chemische Industrie*, Dr. Weyl points out the faults of these methods, and recommends, as preferable, decomposition of the fuel by dry distillation and analytical determination of the solid, liquid, and gaseous products of decomposition. In this method the accident of too small a sample being used is avoided, as also too great pulverization and drying at high temperature and the decomposing action of atmospheric oxygen, which is therewith connected, and the whole of the coke is weighed, and its carbon, hydrogen, and mineral constituents determined. The water, tar, and gas that are formed are measured, and their heat of combustion ascertained with the aid of data that have been supplied by Favre and Silbermann and Deville. The final result will, of course, exceed the true combustion value of the coal by the amount of heat equivalent to the work of decomposition into coke, tar, and gas. The decomposition of the coal should be done as quickly as possible, and at a high temperature.

CONFIRMATION OF THE DISCOVERY OF THE PLANET VULCAN.—In a communication addressed to Rear-Admiral Rogers, Supt. of the U. S. Naval Observatory, under the date of August 2nd, Prof. J. C. Watson, of Ann Arbor,

confirms his reported discovery of the interior planet, to which we alluded in last week's issue, in discussing the successful results of the late eclipse expeditions. The letter contains, likewise, a summary of the observations upon which the announcement of the discovery is based, and which, coming from so accomplished an astronomer as Prof. Watson, leave no reasonable doubt as to their genuineness and of the accuracy of his inferences.

With Mars' moons, and the long-sought-for Vulcan, as the contribution of America to this department of science within two years, our astronomers have earned more than their share of triumphs. The letter is as follows:

"I have the honor to report that at the time of totality I observed a star of the four-and-a-half magnitude, in right ascension, 8 hours, 26 minutes declination, 18 degrees north, which is, I feel convinced, an intra-mercurial planet. I observed with a power of forty-five, and did not have time to change the power so as to enlarge the disk. There is no known star in the position observed, and I did not see any elongation such as ought to exist in the case of a comet very near the sun. I will hereafter report to you more fully in regard to the observations made. The appearance of the object observed was that of a ruddy star of the four-and-a-half magnitude. The method which I adopted prevents the possibility of error from wrong circle readings; besides, I had memorized the Washington chart of the region, and no such star was marked thereon. By comparison with the neighboring stars on Argelander's scale, the magnitude of the planet would be fifth, although my direct estimate at the time of the observation was four and a half, as stated."—*Polytechnic Review.*

NEW FIRE ENGINES.—The Metropolitan Fire Brigade have just added to the plant of the new chief station, in the Southwark Bridge road, two of the most improved form of light steam fire-engines, specially suited for rapid transmission to a fire. These engines were tested on the premises of the makers, (Messrs. Shand, Mason & Co.), in the presence of Captain Shaw and his officers. Various improvements have been introduced; by means of those in the boiler, steam was raised from cold water to 100 lbs. on the square inch in 6¼ minutes, this being an acceleration of time by about three or four minutes as compared with the engines previously in use—a most essential point, considering the necessity of bringing a jet of water to bear upon the fire in the shortest possible time. The increasing height to which warehouses and public buildings are now carried in London rendering it necessary for increased pressure in the water jet, has been met in these engines by an increased area of steam cylinder as compared with the water cylinder, while the difficulty of the man in charge of the jet being able to shut it off entirely to avoid unnecessary damage by water, or from other causes, without the roundabout way of sending a messenger to the engine, which may be in another street, has been met by the adoption of a patent self-acting apparatus by which the jet may be entirely closed at the building on fire without interfering with or stopping the working of the engine. This is accomplished by a special hydraulic safety-valve regulated by a spring balance, which allows all excess of pressure to be relieved by passing the water to the suction-pipe. The first of this improved form of engine has been sent by the makers to the Paris Exhibition.

IMPORTANCE OF GEOLOGICAL KNOWLEDGE TO ENGINEERS.—The value of at least an elementary knowledge of geology to the engineer cannot be over estimated. It is applicable in nearly every work upon which he may be engaged. In the projection of earthworks, tunnels, drainage, water supply and the selection of sites for any structure, success depends largely upon geological considerations.

The engineer should be familiar with the laws governing rock deposition and metamorphism; he should know how rocks are fractured; upheaved and faulted; he should know the characteristics of such as enter his work, and he should know their order of succession.

The stability of earthworks depends quite as much upon the character of the underlying rock as upon careful construction. A deep cut may change a natural drainage, and serious results might follow. The trickling of water through a severed bed of marl or sand may produce a serious earth slip. The dip of the beds should be ascertained.

Railroad and canal embankments and cuttings could oftentimes be more wisely located at a great saving of cost. True, circumstances may compel their location at points not geologically economical, but the engineer who can foresee the evils that might follow from such location, will best be enabled to prevent disaster. Enormous expense has attended repairs resulting from the ignorance or neglect of such anticipation. More than $100,000 were required to remedy the slips in the Breval cut (3000 ft.), on the Paris & Cherbourg railway.

In tunneling, a knowledge of stratification is absolutely necessary, for without it no true estimate can be made. Even the genius and skill which projected that grand work, the St. Gothard tunnel, have had their brilliancy clouded by the blundering miscalculation of its cost. Want of thorough geological inquiry seems evident.

The engineer should know what probable rock will be found at a certain depth, whether or not water may be expected, and if so, under what pressure.

The location of reservoirs should not be determined by merely superficial observation. The suitability of the underlying stratum should be settled. The fact that certain rocks allow water to pass freely through them, while others are almost absolutely impermeable, is as important in its application to rocks out of sight as to those at the surface. Land slips teach us this.

More time might be expended economically in the careful examination of the surface rock; it might be permeable without seeming so; it might be connected with a permeable stratum containing injurious soluble matter; or there might be a near limit to its retentive power. A little attention in this direction might be as profitable as good construction.